THE

ECLECTIC REPERTORY,

AND

ANALYTICAL REVIEW,

Medical and Philosophical.

EDITED BY A SOCIETY OF PHYSICIANS.

.... Apis matinæ
More modoque.—Hor.

Nullis unius disciplinæ legibus adstricti, quibus in philosophiâ necessariò paremus, quid sit in quaque re maxime probabile semper requiremus.—Cic.

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Vol. II. OCTOBER, 1811. No. V.

SELECTED PAPERS.

Memoir concerning the Influence which the Nerves of the Lungs possess over the Chemical Phenomena of Respiration. By Jean Michael Provençal, Doctor in Medicine of the Faculty of Montpelier, &c. Read before the National Institute of France, Nov. 29, 1809.*

From the New Medical and Physical Journal for 1811.

THE influence exerted by the nervous system upon all the functions of the animal economy is well known; and it was long ago discovered, that if by any accident, the nerves entering into an organ are lacerated, cut, or violently bruised, that organ soon loses the power of sensation and motion, and ceases to perform its proper functions.

Physiologists have known how to profit by these facts which occur in practice; they have performed experiments upon living animals, and purposely divided the nerves in them, in a manner similar to what happens accidentally in men; thus making use of these experiments as an analytical method of studying, in their various relations, the complicated functions of the human system. This, certainly, is not the very best method to follow in studying the phenomena of life; since it is not very easy to

* Journal Gen. de Medicine.

judge of the functions of an organ while it is deprived of nervous influence, and at a time when the animal is disturbed and terrified at the sight of the instrument and distressed by the pain of the operation. It, however, demonstrates sufficiently well the influence of the nervous system on the generality of functions; and if the researches carried on in this way do not always lead to important conclusions, they bring to light certain facts which explain many interesting phenomena. Besides, when we make inquiries into operations so complicated, and into functions so essential to the maintenance of life, it behoves us to consider them under every different point of view, and to employ all sorts of experiments to elucidate their history.

Tying the nerves produces the same effects upon the organs to which they are distributed, as a complete division of those nerves. It destroys all communication between the brain, the spinal marrow, and the organs; impedes the nervous influence, and even intercepts the action of the Galvanic fluid, as M. Humbolt first observed. This philosopher found, that after having cut the nerve of the leg of a frog, and separated it from the surrounding parts, to the extent of a finger's length, if he tied the nerve on a level with the parts on which it was ramified, or, if he covered the portion of nerve included between the ligature and flesh, so that it was not in contact with the air, and afterwards exposed the extremity of the nerve to the galvanic apparatus, he could not perceive in the limb to which the nerve belonged, any effect from the action of the galvanic fluid. Organs destined for any particular secretion, are also affected by tying or cutting the nerves which connect them with the brain or spinal marrow. M. Dumas* observed long ago, that the secretion of the gastric juice was speedily diminished upon cutting or tying the eighth pair of nerves, and also, that when these same nerves were cut or tied, the solution of the aliments was suspended, until fermentation and putrefaction took place.

From the time of Galen, until the present, physiologists have at different epochs, and with various intentions, made ligatures upon, and cut the two nerves of the eighth pair, or sometimes some of its branches only, in order to observe the effects pro-

* Dumas, Principes de Phisiologie, tom. 1, 2d edition.

duced by the interruption of the action of the brain upon the organs to which those nerves are distributed. But no person before M. M. Dupuytren, Dumas, and Blainville, thought of ascertaining what effects dividing those nerves would produce upon the phenomena of respiration, and upon the colour of the arterial blood. M. Dupuytren* first engaged in this inquiry, and in a course of very curious experiments, he observed, that a section of, or ligature upon, the eighth pair of nerves, was always mortal; that dividing them brought on asphyxia; that the arterial blood became dark, and almost of a coal-black colour; that during this asphyxia, the air still continued to enter into the lungs, and the blood to circulate through them; that he could, by means of simple compression of the eighth pair of nerves, bring on asphyxia, and suspending or continuing the compression, could remove the asphyxia, or render it at length fatal. From all these facts, this able surgeon rightly concluded, that respiration in a healthy state is carried on by the influence of the nerves distributed to the lungs, and by that of the nerves of the brain.

M. Dumas, who has enriched physiology and medicine with many valuable experiments and useful discoveries, repeated these experiments, and made some other ingenious ones in order to explain these facts, and he has established the following propositions.

1. The disturbance which pain produces in the respiration is sufficient to change the red colour of arterial blood; it renders it black, in the same manner as dividing the nerves which belong to the lungs, because, in consequence of the derangement produced in these organs by the pain, the air does not sufficiently penetrate them to act upon the blood, and to colour it red.

2. The arterial blood does not become black immediately upon the nerves being divided; it does not take that colour until all the air contained in the interior of the lungs is totally absorbed.

* Expériences touchant l'influence que les nerfs des poumons exercent sur la respiration. Biblioth Méd. 1807.

3. After the nerves are divided, and the red colour of the blood has been changed into black, the red colour may be reproduced, by forcibly introducing into the interior of the lungs, by mechanical impulse, oxygen or atmospheric air.

4. In animals which have had the eighth pair of nerves divided, the appearances do not resemble those produced by suffocation from irrespirable gases, but those arising from a total deprivation of air.

5. By the contact of oxygen with the blood in the arterial trunk the chemical action takes place, which colours the blood red, although this chemical action has not been submitted to the influence of the lungs.

6. The colour of the blood being a physical quality cannot be modified by the vital action in the circumstances essential to its production, but only in the accessory circumstances, such as the introduction and diffusion of air through the vesicles of the lungs, where it is placed in contact with the principles of the blood.

M. Blainville has remarked, that the division of one nerve only of the eighth pair is not mortal; that rabbits die in seven hours, and pigeons on the sixth or seventh day after the division of both nerves; he has observed, that the number of inspirations is diminished after the operation, but that as great a volume of air enters into the lungs of the animal after, as before the division; he found that the arterial blood did not pass into the state of venous blood, and he could not discover any manifest sign of asphyxia. The air inspired, says M. Blainville, appears to be vitiated in the same manner after, as before the operation, whence we may conclude, that the chemical phenomena are not interrupted.

We see by this short account of the experiments of M. M. Dupuytren, Dumas, and Blainville, that these authors do not entirely accord in many points of the history of this experiment, and that every one of them arrives at a different conclusion. My purpose is, not to inquire into the cause of this diversity of opinion, or to endeavour to determine, from their experiments, and those I have myself made, the real phenomena observable in respiration, and the changes the red colour of the

blood undergoes after the section of a ligature upon the nerves of the eighth pair. I propose in this memoir to examine, whether an animal, having had the eighth pair of nerves cut or tied, absorbs as much oxygen, and produces as much carbonic acid after, as before that operation, and if the animal heat undergoes any variation in the course of the experiment. The solution of this important question will serve to complete the beautiful experiments of M. M. Dupuytren, Dumas, and Blainville.

To resolve the first part of the question, I made at Arcueil, in conjunction with M. Berthollet, a great number of experiments on Guinea pigs and rabbits. This was the method I adopted. Before making the ligature or section of the eighth pair of nerves, I placed the animal in a *manomètre** in a given capacity; I ascertained exactly the quantity of oxygen absorbed, and of carbonic acid produced in a given time, while the animal was in a state of perfect health, that I might be able to discover the changes afterward produced in the chemical phenomena of respiration. I then tied or divided the nerves in this animal, and a little time after, placed him in the same manomètre, where I allowed him to remain the same length of time as he did before the operation. I must observe, that the manomètre, I made use of in these experiments, was not provided with a thermometer within side, and that I was obliged to ascertain the temperature before the animal was taken out, by applying the bulb of a thermometer to the manomètre. But this slight inconvenience did not affect the accuracy of the experiment, because it was only necessary to mark the relative quantities. I found the quantity of carbonic acid, by washing the gas with lime or barytes water. All the analyses were made by burning hydrogen gas, after the method of M. M. Humboldt and Gay-Lussac. As it would be tedious and unnecessary to detail every experiment, I have given in the following table the results I have obtained.

* See a description of this instrument by M. Berthollet, in the first vol. of the Society of Arcueil.

Animals submitted to the Experiment.	Time employed in the Experiment.	Analysis of the Air in the Manometre.			
		Parts in 100 of oxygen absorbed.		Parts in 100 of carbonic acid produced.	
		Before cutting the nerves.	After cutting the nerves	Before cutting the nerves.	After cutting the nerves.
A rabbit	20 minutes	9,86	6,34	6,91	4,62
A rabbit	20 minutes	10,01	6,72	7,80	5,01
A rabbit	20 minutes	9,47	8,02	6,86	5,43
A Guinea pig	20 minutes	7,92	5,81	6,13	3,07
A Guinea pig	20 minutes	7,10	5,98	5,80	4,01

It appears from these few experiments contained in the table, as well as from all the others I have made, that animals, after having had the eighth pair of nerves tied or cut, constantly absorb less oxygen, and give out less carbonic acid than before the operation. This difference in the chemical action of respiration, the effect of the division of the nerves, is at first but little evident, but in proportion to the time elapsed from the operation, the animal always consumes less oxygen, and the quantity of carbonic acid varies in like proportion; and at length, all the chemical phenomena are suspended, cease entirely, and the animal dies.

On opening the body, the lungs are found more or less loaded with black blood. This accumulation does not take place in the last moments of life, as one might suppose; I have opened the thorax of dogs ten or twelve hours after the operation, and while they were yet strong enough to live several hours, whereby I ascertained this fact. It does not take place in animals who die *very shortly* after the section of the nerves, as I have observed in a great number of rabbits and Guinea pigs.

It would certainly be of no little importance, to be able to explain how death takes place in animals in whom the eighth pair of nerves has been tied or cut, and to detail the successive phenomena under which they die; but as this examination would lead me too far from the purpose of the present essay, I shall make that the subject of a second memoir, in which I shall

relate the facts I have observed, in going over the observations and opinions of all the physiologists who have practised this experiment.

As it has been clearly demonstrated by the experiments of Lavoisier and M. Laplace,* that respiration is the principal source of animal heat, and that the temperature of animals depends upon the state of their respiration, it became very interesting to determine if the section of the eighth pair of nerves, which produced so sensible a diminution of the quantity of oxygen which an animal absorbs, and in the quantity of carbonic acid expired in a state of health, would not also produce a difference in the temperature of animals.

In order to determine accurately if the heat of animals underwent any alteration, after the section of the eighth pair of nerves, it was first necessary to ascertain, by a great number of observations, the degree of temperature in the animals destined for experiment, and to compare the results carefully with those obtained after the section of the nerves. But how should we proceed to arrive at an exact knowledge of this temperature? Some physiologists have introduced the bulb of a thermometer into the mouth, ears, and rectum of the animals, whose degree of heat they wish to know. But it appears to me, that this mode of taking the temperature of animals is not very correct; it only gives us the specific heat of the parts themselves. Thus, in opening the mouth of an animal to place the thermometer there, the continual movements of inspiration and expiration, produce in that cavity currents of air which prevent the real temperature being taken. M. Prunelle, in observing the temperature of hedge-hogs, found that the thermometer placed in the mouth indicated the heat to be two-fifths less than the actual temperature of the animal. Other physiologists have applied the bulb of the thermometer to different parts of the body, but this appears to me to be still less exact; for we know, that many circumstances may render the circulation less active towards the external parts, concentrate the vital force inwardly, and speedily change the temperature on the surface of the body.

* Memoirs of the Academy of Sciences, 1781.

The sanguiferous system, and more particularly the arterial canals, being the principal centre of animal heat, it is into one of these large vessels we should plunge the thermometer, if we would ascertain the temperature of animals.

That was the method I employed in all my experiments; it has this inconvenience, that by the speedy death of the animal, the physiologist is prevented from again ascertaining the temperature after the section of the nerves, so as to estimate the change the section has produced in the heat. But as I could easily procure as many dogs as I wished, I remedied this defect by always choosing two dogs of the same size, as nearly as possible of the same age, and of the same sex; I opened the chest of one, and puncturing the aorta near its origin from the left ventricle, I introduced into it the bulb of a thermometer; I then divided the eighth pair of nerves in the other dog, and after some hours, opening his thorax, I punctured the aorta or left ventricle of the heart to ascertain his temperature. I acknowledge, however, that the circumstance of not being able to make both observations on the same animal, leaves some doubt as to the slight modifications the age and temperament of different individuals of the same species might cause in each individual; but I think these modifications are too slight to form any solid objection to the facts I have the honour to lay before the Institute.

I always observed some hours after the operation, that the temperature was less in the dog, in which the eighth pair of nerves had been divided, than in the other dog not operated on. To determine positively whether this diminution of animal heat was at all owing to the wound in the neck, to the accidents occasioned by it, or to the division of the nerves, I exposed to view the nerves in a dog, without dividing them; at the same time, I divided the eight pair of nerves in another dog, and after a certain period I opened them both; I always found that the dog which had merely the nerves exposed by several incisions, was of a higher temperature than the one whose nerves had been divided, and that the heat was equal to that of a dog which had not undergone any operation.

It would be extremely important, to compare these results

with those which would be obtained by making use of a calorimeter. This machine is extremely convenient, and very accurate. One can determine by it with the greatest precision the degrees of alteration the animal heat undergoes from the moment of dividing the eighth pair of nerves, until the animal dies. I shall very speedily make these experiments; I shall compare them with those made by the thermometer, and I shall give an account of them in a work I mean soon to publish, on several matters relating to the history of animal heat. The following table contains the results of my experiments.

Animals opened to ascertain their temperature.	Degrees of the thermometer, (centigrade.)	Animals opened some hours after the division of the eighth pair of nerves, to ascertain their temperature.	Degrees of centigrade thermometer.	Animals opened some hours after the simple exposure of the eighth pair of nerves, to ascertain their temperature.	Degrees of centigrade thermometer.
Ordin. sized dog	39,4	Ordin. sized dog	38,0	Ord. sized dog	40,0
ditto	39,5	ditto	37,2	ditto	39,7
ditto	39,7	ditto	37,5	ditto	40,0
ditto	40,0	ditto	38,1	ditto	39,8
ditto	40,0	ditto	37,0	ditto	40,1
Very large dog	40,0	Very large dog.	37,4	ditto	40,0
ditto	39,9	ditto	37,3	Very large dog	39,9
ditto	40,0	ditto	37,9	ditto	40,0
ditto	40,0	Large pregn. bitch	38,0	ditto	40,0

It appears to me very probable, that the diminution of animal heat, after the division of the eighth pair of nerves, is essentially owing to the alteration produced in the lungs by this division, and particularly to the animal's taking in less oxygen, and producing much less carbonic acid than in a natural state. We know indeed, that dividing the nerves of any muscles will bring on paralysis of those muscles. We are not ignorant that the lungs receive branches from the great sympathetic; but it would appear that the eighth pair exercises considerable influence upon these organs. The lungs, in truth, do not partake of the nature of muscles, but they may be compared to them in respect to the motions they perform, and the necessity of their being supplied with nervous influence, to enable them to exercise their functions properly.

We may, I think, deduce the following propositions from all the facts related in this memoir.

1. Respiration is performed in a natural state, under the influence of the brain, conveyed by means of the eighth pair of nerves.

2. The chemical phenomena of respiration are not destroyed after the division of these nerves; they only become weaker in consequence of the alteration produced in the lungs by this division.

3. Animals, on which this operation has been performed, consume a less quantity of oxygen, and produce less carbonic acid, than in a state of perfect health.

4. The temperature of the dogs opened in these experiments was often above forty degrees of the centigrade thermometer.*

5. When the nerves of the eighth pair were simply exposed to view, the dogs often preserved their natural temperature during the first twenty-four hours.

6. Those, on the contrary, which had the nerves divided, were of a much lower temperature some hours after the division.

A Case of Dysphagia, together with some other unusual Affections, supervening an Inflammation of the Lungs, wherein a Gum Elastic Tube was advantageously employed as a passage to the Stomach. By Edward Barlow, M. D. Member of the Royal College of Surgeons in Ireland, &c.

From the Medical and Physical Journal, for March, 1808.

It frequently happens in the practice of medicine, that a physician's embarrassment arises, not so much from the paucity of his remedies, as from the difficulty of selecting judiciously from among the multiplicity which present themselves to his choice. On the contrary, it but too often occurs, that every means of giving relief are ineffectually exhausted; and that the physician is doomed to experience that keen distress, which must ever attend the consciousness of inability to avert impen-

* 104 degrees of Fahrenheit.—Ed.

ding dissolution. In this latter situation, every auxiliary is valuable; and with a view to extend the knowledge of, and confidence in, an important one, I trouble you with the present communication.

A lady of this city, whose age was 67, was seized, in November, 1805, with symptoms of slight pulmonic inflammation, which readily yielded to blood-letting by leeches, to blisters, and small doses of tincture of digitalis, in conjunction with the other usual treatment. The febrile symptoms had declined, and the several others had gradually subsided. The pulse was reduced from 110 to 86, and perfectly regular; appetite was restored; and a degree of alienation of mind, which had occasionally occurred, particularly during the hot stages of the febrile exacerbations, gave place to perfect intelligence and tranquillity. Under such favourable circumstances, a speedy restoration of health was naturally expected. On the morning, however, of Saturday, the 23d day of November, this being the seventh or eighth day of her confinement, the fourth of medical treatment, she was awakened at four o'clock by a sense of suffocation, and of inability to respire freely. By unaccountable neglect, I was not apprised of the change until twelve o'clock at noon, the hour of visiting, which I had appointed on the preceding evening, when wholly unsuspicious of any sinister event intervening. I found her much distressed by suffocation, but collected in mind, and perfectly adequate both to understand others, and to make herself intelligible, although unable to articulate many words in succession. This inability manifestly proceeded from the muscles of voice being either unequal to exertion, or from some other cause, disobedient to her call; and it is not unworthy of remark, that, both at this time, and throughout her subsequent illness, French words could be pronounced with apparent facility when the correspondent English words could not be articulated. I learned from her, that she suffered no pain; had little or no cough remaining, nor any complaint, save the suffocation which I witnessed. This seemed attended by convulsive catchings of her breathing; the returns of which seemed dependent on the recurring necessities of the lungs for air, which the muscles of respiration, either from quiescence or

irregular action, failed in their agency regularly to supply. The appearance was truly singular; the convulsive catchings being compounded of singultus and dyspnœa, or, in other words, being referable to irregular actions of the ordinary muscles of respiration conjointly with those of the diaphragm. These seemed to fall into irregular actions, not from a recurring disposition to such actions, as is the case in the ordinary instances of clonic spasm, but merely from the demands of the system for a renewal of the air contained within the lungs; for when by such convulsive effort fresh air was inhaled, the muscles seemed immediately to return to a state of perfect inaction, apparently the result of exhaustion and extreme debility, and to continue so until again aroused by a repetition of the same excitement; a term which I here employ solely to express that indirect influence which the necessities of the system appeared, by long established habits of association, to exert over the respiratory organs: by which influence alone these seemed, at this time, to be aroused to any exertion; extreme debility having rendered them nearly incapable of any.

As the period was critical, and the danger manifest, I called immediately for further assistance; and almost instantaneously procured the cooperation of a highly judicious physician, by whom this lady's family had been occasionally attended.

We immediately agreed on the propriety of exhibiting the camphor julep, combined with volatile fetid spirit; and applied a large sinapism, without delay, to the fore part of the chest. By these she was so much relieved, that in the evening she took some bread and coffee with appetite, and seemed nearly free from spasm or suffocation. In order, however, to preserve the ground we had gained, and to guard against a return of complaints, boluses of musk, ammonia, and camphor, were ordered in addition to the former mixture, which was to be continued; due attention being also given to the essential object of nutrition.

This night she slept well, and the next morning (Sunday, 24th November) seemed refreshed. She took breakfast with appetite, but seemed not free from occasional spasms of the organs of respiration; or rather she seemed still unable to em-

ploy the respiratory muscles in that regular and continued exercise essential to free and perfect respiration. Pills were now substituted for the boluses; these being found difficult to swallow. In the evening she grew worse, the suffocation increased, and the cough became troublesome, but without any pain, and to all appearance influenced only by the unexpectorated mucus. A sinapism was ordered, and the pills and mixture were continued. It became necessary too, at this time, to order an enema; and from the tranquillizing effects she formerly experienced from the pediluvium, she was herself desirous of having this repeated, which was ordered accordingly; she also again took bread and coffee with appetite. On the following morning (Monday 25th) I visited her very early, and found her breathing still laboured, but free from pain; her pulse remained, as throughout, perfectly regular, and of good strength; she had much cough, with free expectoration, which was difficult, however, from weakness and inability to expectorate.

At eleven o'clock, A. M. deglutition became much impeded, and there was a manifestly progressive debility and tendency to spasm, extending upwards to the muscles both of voice and deglutition.

Wine was now administered, with vitriolic ether, and liquid jelly was occasionally given, in order to restore strength by every means. Notwithstanding which, a little after noon, deglutition became entirely suspended; nor could the power be restored, although the external fauces were stimulated, first with cloths wetted with vitriolic ether, and afterwards by warm oil of turpentine and tincture of cantharides. Enemeta too of assafœtida and tincture of castor were injected, with equal inefficacy.

On this evening, there being no amendment, while the strength was rapidly declining, the anxiety of the patient excessive, the throwing about of the arms almost incessant, and, in short, every prospect before us of speedy dissolution, we yet thought it right to procure the assistance of some other practitioner; and a physician of eminence was accordingly called in, by whose advice we put sinapisms to the legs, and blisters to the soles of the feet.

The next morning (Tuesday 26) I was deprived of the aid of that physician, who had attended with me from the onset, he being obliged to leave town. The physician, whom we last called in, however, visited with me at ten o'clock, when we had the mortification to witness a deterioration, in every respect, since the preceding evening. We found the respiration effected only by a species of singultus, and at considerable intervals; the pulse rapidly sinking; deglutition totally suspended; voice nearly lost, being capable only of uttering inarticulate monosyllables; and a countenance expressively indicative of approaching death. Still she remained perfectly sensible, was conscious of her situation, and resigned to the expected event.

Why, under such apparently hopeless circumstances, any further trials were thought expedient, may here require some explanation. In the first place, I was well assured, from the progress of the disease, and the decided concurrence of both my medical friends, that no pulmonic disease existed at this period; and that there was no ailment to contend with, save debility alone. I was satisfied that mere debility need not necessarily destroy life, so long as adequate stimulants could be employed to excite the vital powers to action; I felt that our efforts to introduce both stimulants and nutriment, had been fatally interrupted by the suspension of deglutition; and I decided in my own mind, that if by artificial deglutition a sufficient stimulus could be applied to the stomach, which organ presented the only medium through which the general system could be effectually excited, that life might even yet be preserved. With this view, I had proposed, the preceding evening, the employment of a flexible tube, as a conveyance to the stomach; and had canvassed with that medical friend, whose assistance had been withdrawn from me, the propriety of using such in the present case, and had received his unqualified assent, and warm approbation of the trial, as being the only means which offered any reasonable prospect of giving relief. This proposal I now repeated, and was pleased to find, that the gentleman, to whose judgment I submitted it, did not dissent; although, from the extreme hopelessness of the case, from his being unacquainted with any similar instance, wherein such an expedient was

adopted; and from some apprehension lest the attempt to introduce the tube might bring on general spasm, which might prove fatal, and be the immediate precursor of death, he did not cordially coincide. I had no occasion to dwell with much force of argument on the advantages of introducing food and stimulants to the stomach; but, with respect to the objections, I pleaded that the trial was not a new experiment, as it had been repeatedly made, and with the best effects, in cases of dysphagia; that the absence of every disease, save debility, rendered the present case singularly favourable for such a trial; and, that there was little danger of exciting spasm, either local or general, by the introduction of the tube; for that, in general, it was rather by titillation, than by rougher handling, that the muscles of the fauces were thrown into irregular action; that in cases where pins were swallowed, or other extraneous matter lodged in the œsophagus, surgeons were in the constant practice of introducing *probangs* even as far as the *cardia*, without even vomiting being excited; and further, that there seemed no peculiar disposition to spasm in the present case; inasmuch as those actions, which we witnessed, were not the result of an irritable state of the muscles, so much as of a directly opposite state, a state of atony; for that they were, even with difficulty, excited to perform their necessary and accustomed actions, by the constantly recurring necessity for inspiration, and the occasional voluntary exertions to articulate; whence I inferred, that no unpleasantness of this nature would attend the operation. To all this, my medical friend yielded a reluctant assent; he agreed in its being the only expedient remaining; wished me success; and, by taking up his hat, expressively indicated his intention of not assisting in the trial; and, by this movement, effectually repressed the request for his presence and assistance, to which I was about to give utterance. He took his leave, and left me to the free exercise of my own discretion.

Embarrassing as this situation undoubtedly was, and particularly to a young practitioner, I yet resolved not to abandon the trial, but to give this last and only chance, to my old and very dear friend and relative, whose fate I sincerely lamented.

To this I was even urged by her friends, from their confidence in me, and from the simple explanation I was enabled to give of the proposed expedient. And it may serve to elucidate how far, in similar circumstances, steadiness of purpose, and conviction of the propriety of his measures, on the part of the adviser, may contribute to overcome prejudice or opposition, when I declare, that a ready assent was afforded to my proposition, at a time when by some strange misconception it was even understood, that the gum elastic tube was to be introduced by an artificial opening, instead of by the natural passage.

In defiance of all discouraging circumstances, I was about to proceed to the introduction, when, to my great satisfaction, I received a message from the late Dr. Purcell, whose assistance we had not been able to procure the preceding day, that he was now ready to attend, if required.

I joyfully availed myself of such valuable aid, and had soon the opportunity of detailing to him the history which I have here given. He suggested some trivial remedies; but with a candor, which I shall ever remember, he at once acknowledged, that he could propose nothing so likely to succeed, as the introduction of stimulants to the stomach by an artificial conveyance; and, after listening to my arguments and explanations, he without hesitation agreed to witness and assist me in the trial. I accordingly, under his inspection, introduced the gum elastic tube with perfect ease, and with little disturbance to our patient, while he poured in as much diluted alcohol as he deemed advisable. The quantity, thus introduced, did not I think exceed half an ounce of a mixture containing equal parts of alcohol and water; and this he was unwilling to exceed, especially as he saw with what ease its exhibition could be repeated. After this the abdomen was fomented with warm spirits, and an enema of broth, with opium, was administered. A slight exhaustion seemed to ensue from the trifling fatigue of this inconsiderable exertion, which was soon followed, however, by a general excitement of the whole system, an increase of strength in the pulse, and more regular efforts at breathing. She lay quiet for about an hour, when, from the amendment

throughout her entire appearance, and the total alteration which had taken place therein, I was induced to attempt introducing some warm wine into her mouth, with a view to her swallowing it; and, after a short interval, was delighted to find it actually swallowed, by a perceptible muscular exertion. I continued to give wine by spoonfuls, and was happy to perceive them swallowed with progressively increasing facility; at length, to my utter astonishment, she actually took the cup from my hand, and, by her own effort, raised it to her head, and drank off its contents.

This soon produced some slight aberration of mind, and a degree of incoherence, the manifest effect of a transient inebriety. It soon ceased, however, and on it supervened a sound and refreshing sleep, which lasted several hours, and from which she awoke apparently free from every complaint. She now with firm voice and connected expressions *asked* for wine, which was given her in conjunction with some liquid jelly, and which she took with considerable appetite. Broth enemata were repeated and retained; fomentations were again used, and she again slept soundly. On awaking the second time, the vigor both of mind and body which she evinced, astonished all her freinds who witnessed it. She spoke to them severally, inquired into their health, and seemed highly gratified, both with the attentions she had received, and with the prospect of recovery, of which she had formerly desponded. Her head-dress being completely unsettled, by the jactations of her body during the preceding days, she felt anxious to have it arranged; and, as it was likely to contribute to her better repose, she was indulged. During this short process she displayed a considerable degree of liveliness and humor; and, with a *naiveté* of manner, with which she was singularly gifted, she repeated some well known lines of Pope's, on female vanity, with precision. Her head-dress and bedclothes being settled to her satisfaction, she wished for food; took some biscuit and coffee; expressed a desire to sleep, and, with that view, turned herself on her side, contracting all her limbs into the semiflex position usual with those in perfect health. Her sleep was calm and undisturbed, and continued even to seven o'clock

next morning, (Wednesday the 27th,) when I was again called. Finding, however, that she had taken both wine and jelly during the night, and that a broth enema had also been given and retained, I was unwilling to disturb her, conscious that sleep was more likely to prove restorative than any means I could then employ. I returned again at ten o'clock, when I had to encounter the shock of hearing that all my hopes were fallacious, for that my friend had just expired.

Scarcely crediting the information, I entered, and found the body still warm, with a pulsation distinctly discoverable, both at the heart and wrist. Urged by the suggestions of the moment, I again introduced the tube, and poured diluted alcohol and ether into the stomach.

The stupe too, being in an adjoining chamber, I had cloths wrung from warm spirits applied to the external stomach, the abdomen, and extremities, but in vain.—At length, the spirit was fled, and not to be recalled.

It appeared, on inquiry, that her sleep had for some time become disturbed and uneasy, but her attendants, conceiving it injudicious to awake her, though impelled by their own feelings to do so, had suffered her to continue in this state. A few minutes before ten o'clock she awoke, apparently much agitated; breathed by short and hurried inspirations for a few moments; then breathed her last without a struggle.

It is scarcely required to offer any comments on the facts here stated; it may be right, however, to mention, that they are accurately detailed from notes regularly registered at the time of the occurrence.

The propriety of substituting an artificial mode of deglutition, when the natural had so entirely failed, can admit of little doubt,—and its success, as far as regarded the mere trial, must to all appear complete. That life was not ultimately preserved, in this case, can offer no reasonable objection; for this fatality can only lead us to infer, that, from age, and extreme delicacy of habit, the constitution was here so nicely balanced, so accurately adjusted, as though equal to the preservation of life in health, to be unable to contend with even the slightest degree of disease; and, notwithstanding the removal of disease was survived, yet, that the vital powers, exhausted by the effort,

were incapable of keeping up, by any aid of stimulants, that degree of excitement necessary to the continuance of vital action. Perhaps too, we are called on to admit, that a favourable opportunity for further continuing the necessary excitement was lost, by the timidity of the attendants, who, when sleep became disturbed, and a manifest degree of agitation had commenced, would have acted more wisely by awakening her, and introducing additional wine and sustenance. With respect to the means, by which so much was accomplished, I have no hesitation in recommending its adoption in every case wherein suspended deglutition puts a stop to nutrition and medical treatment; nor do I see why its use should be confined to chronic affections, and not be extended to the latter stages of acute diseases. The expedient is simple in its nature, perfectly harmless, and applicable by any one possessed of the slightest knowledge of the parts concerned; nay, even they who are utterly ignorant thereof, may yet employ it with safety.

Many instances of its employment, in dysphagia, are on record; and it was from recollecting one of these, detailed by the celebrated John Hunter, that I was induced to inforce the use of it, in this case, with so much confidence. The case I here allude to, was one of dysphagia from paralytic affection of the œsophagus; and, from the means thus afforded of sustaining life, and of employing medical treatment, the disease was removed, and the patient ultimately recovered. Of the eligibility of the trial, in the present case, no doubt whatever was expressed, and of its perfect safety I had myself the fullest conviction. I trust it will appear, that I was both warranted therein by previous reasoning, and justified by the result. I further hope, that, in thus giving publicity to this detail, I do no disservice to the practice of medicine.

Conscious of the latitude which private records of diseases give for undetected embellishment, and knowing well with what distrust the unsupported assertions of individuals are oftentimes received by the public, I have deemed it incumbent on me to submit these pages to the inspection of that medical friend, who had witnessed this singular case during a principal part of its progress; and am now authorized to give, in addition to my own authority, the respectable evidence of Dr. Toole,

both as to the authenticity and correctness of most of the preceding statements.

To him I am indebted for much valuable assistance during this trying attendance; and shall be, at all times, happy to express the respect which I feel both for his professional and general character. The latter part of the history, however, must rest on my own credit alone; as the corroboration of those, who witnessed it with me, could not well be asked for; and as the opportunity of adducing a still higher authority, in confirmation of this part of my statement, is unfortunately drawn from me, by the death of the late Dr. Purcell; a man, whose professional eminence was justly merited; whose urbanity of manners, and intelligence of communication, will long be remembered by all who had ever the opportunity of appreciating their value; and whose loss, every admirer of genuine worth and talents will join me in lamenting.

EDWARD BARLOW.

April 7, 1807.

Memoir on the Cause of the Refrigeration observed in Animals exposed to a great heat.

By FRANCIS DELAROCHE, M. D.

Read at the meeting of the first Class in the Institute on the 6th of November, 1809.

Translated for the Eclectic Repertory, from the Journal de Physique.

THE animal economy presents some phenomena, which differing in their nature from those observed in unorganized bodies, cannot be explained by the laws of dead matter. It however presents others which resemble more or less known physical effects, and which appear to result from the same laws. Some physiologists, aware of the errors arising from the desire of attributing every thing to mechanical causes, will not admit of any explanation of this kind in the animal economy. They imagine that the phenomena essentially connected with the vital action must depend upon the laws which govern life,

and not on mechanical principles, which have little relation with, and frequently appear opposed to the first: but is not this opinion rather the offspring of speculation than of experience? and because some of the phenomena of life seem incompatible with the laws to which inanimate bodies are subject, must we conclude that all are in the same predicament? This reasoning, faulty in itself, militates against experience. Can any one deny the influence of physical causes, in many of the phenomena of the animal economy, such for instance as distinct vision, which essentially depends on the refractive powers of the humors of the eye; or the mechanism manifest in the motion of our limbs, in which the bones act as levers, and the tendons as cords, &c? It is true that physical causes are not sufficient of themselves without the concurrence of the vital causes to produce these effects.* Their influence is however not the less evident. In general we may pronounce that there is scarcely a phenomenon in the animal economy which may not be referred to one or other of these causes. At one time the influence of the physical causes is the most obvious, at another the vital predominate; and often it is difficult to determine precisely what belongs to the former, and what to the latter. But this is an important object; and the researches conducting to it ought to be ranked among the most interesting in physiology.

If we ever acquire precise ideas respecting the vital causes and the differences which separate them from the mechanical powers, it must be by observing what properly belongs to them in the exercise of life, and not by indiscriminately ascribing to them all the phenomena exhibited by organized bodies.

A case in which it is easiest to make such a distinction is, as it appears to me, presented by animals exposed to a great heat. It is well known that they assume a tempera-

* In speaking of the vital laws and causes, I do not mean to assert, that they are really different or independent of the general laws which govern dead matter. They may be mere modifications. But in the actual state of science they must be admitted if we wish to acquire any tolerably precise ideas of the manner in which the various functions in organized bodies are performed. We are far from being able to refer to physical laws many of the phenomena which these bodies present.

ture much below that of the surrounding medium. For nearly half a century this remarkable property had been observed in animals, and since that time has given rise to many inquiries, and particularly to those made by Sir Joseph Banks, Dr. Fordyce, Sir Charles Blagden, and other philosophers. But as yet no precise ideas have been acquired respecting the cause, which some have ascribed to the cold produced by evaporation of the perspired fluids, while others on the contrary have thought it must be the same as that of animal heat. Some reflections on this question will form the subject of the present memoir; but I will previously state an observation which I made some years ago.* It is that a very exaggerated notion is entertained in supposing that the power of producing cold is as well marked in animals, as that of generating heat. I think I have proved that the opinion generally received since the publication of the experiments of Messrs. Fordyce and Blagden, is erroneous. In a number of experiments made in conjunction with my friend Dr. Berger, I constantly observed that the temperature of animals exposed to a heat above† 95 or 104 degrees, Fahrenheit, rose in a remarkable manner, though it never equalled that of the surrounding medium. I often observed that this elevation of temperature extended 12 or 14 degrees; and I have satisfied myself, that when the external heat is very great, this increase of temperature in the animal has no limits but in death, which necessarily follows. In these experiments I ascertained the temperature of the animals in the most exact manner by means of a thermometer, having a very small bulb, introduced far up the rectum. I have likewise ascertained in man, by a thermometer placed in the mouth, a similar elevation of temperature, and once very decidedly, where the head could not be affected but through the

* In my inaugural dissertation intitled "Experiments on the effects produced by a great heat in the animal economy. Collection of Theses in the Medical school at Paris. 1806, No. 11."

† We have taken the liberty of introducing Fahrenheit's scale, believing that it would be more acceptable to the generality of our readers. Some small fractional differences were unavoidable. Ed.

medium of the circulation; the body being inclosed in a case filled with hot vapors, while the head was exposed to the open air.

Hence it follows, that the power of producing cold is much more restricted, than is generally believed, though it is by no means imaginary. Its existence is attested by too many facts to admit of doubt. It is desirable, therefore, that the cause should be determined, which I shall now attempt.

I have already stated the opinion entertained by some, that this cause must be the same as that of animal heat. It is founded on the experiments of Fordyce and Blagden, from which it might be inferred that animals preserve a uniform temperature whatever may be the heat of the surrounding medium, and that consequently the power of producing cold was as well marked in them as that of producing heat. Indeed, if this were the case, it would be natural to view in this uniformity of temperature, only one and the same phenomenon derived from a single cause. The fact however not being correct, as shown above, the conclusion may be deemed not more so.

This latter opinion receives confirmation from the observation, that in cold-blooded animals the power of preserving a temperature below that of the medium in which they are plunged, when the latter is raised, is as much or more marked than in warm-blooded animals; whereas if this power depended upon the same cause as that of animal heat, it ought to be almost nothing in this class of animals. Indeed I have demonstrated the truth of this assertion by many observations contained in the memoir before referred to. The following experiment, recently made, will, I think, remove all kind of doubts.

I placed a rabbit, whose natural temperature before the experiment was 103 degrees, in a stove at a medium of 113 degrees, Fahrenheit. After remaining an hour and forty minutes, it had acquired a temperature of 110$\frac{3}{4}$ degrees. A frog, exposed in the same stove to a similar degree of heat, had acquired, after an hour, a temperature of 80 degrees, which it preserved for half an hour, the remainder of the time it continued in the stove. The temperature of another frog, exposed to a medium

temperature of $115\frac{1}{3}$ degrees, rose to $82\frac{1}{4}$ degrees, where it became stationary.

Those, who consider that there is not a necessary connexion between the cause of animal heat and that of the cold occasionally observed in animals, have been inclined to ascribe the latter to the evaporation from the surface of the body and the interior of the lungs, thus comparing the phenomenon which engages our attention with the refrigeration produced in unorganized bodies having moist surfaces. But is this hypothesis, for which we are indebted to Franklin, founded on fact? The only experiments hitherto made with the view of resolving this question, to wit, those of Blagden and his colleagues, and those of Crawford seem to show that it is not. The experiments I made some years ago, and of which I gave an account in the memoir already alluded to, inclined me to adopt Franklin's opinion, but did not enable me to come to a decided conclusion. I have since attempted new ones, which confirming the result I had formerly obtained, appear to me to remove all doubts on the subject. I shall state the results, promising a short sketch of those I have already published. The latter were chiefly intended to ascertain the validity of the objection pretty generally urged against Franklin's theory, asserting that the refrigeration produced by evaporation was by no means sufficient to explain the difference observed between the temperature of the animals exposed to a great heat, and that of the surrounding medium. To decide this point, it will be sufficient if we examine the comparative influence of heat on the temperature of animals, and that of dead matter the whole surface of which had been moistened. For this purpose I exposed at the same time and together in the same stove several animals, alcarrazas* filled with water and moistened sponges. In making this experiment which I have frequently repeated, I have constantly observed that the alcarrazas and the sponges, whether introduced cold, or previously heated, into the stove, assumed a temperature below that acquired by warm-blooded animals, but nearly the same

* Porous earthen vessels, through which the contained water transudes so as to keep the surface constantly moist. They are used in Spain and other warm climates for the cooling of water.

as the temperature of cold-blooded animals.* From these results we may then infer that evaporation is sufficient to produce a refrigeration equal or greater than that observed in animals; and we may consequently conclude that it is the cause of the latter phenomenon. It would be wrong however to consider the latter conesquence as necessarily following the antecedent. Though a thing be possible we are not intitled to conclude that it actually exists: moreover when I published these results I did not pretend to assert that evaporation was the true cause of the phenomenon before us. I merely offered it as a plausible conjecture. Now I think I can establish it by direct proofs.

If evaporation be the cause which occasions the refrigeration of animals exposed to a great heat, it is evident that by suppressing it on the surface of the body and the interior of the lungs, this refrigeration will be prevented, and the animals must acquire a temperature equal if not superior to that of the medium in which they are placed. If such a result should not take place, it is a direct proof of the insufficiency of this cause. If on the contrary, the means by which the evaporation is prevented, being such as not to disturb the functions of the animal,

* To have rendered the experiment exact, the final temperature acquired by the animals and the inanimate bodies, when the effect of the heat was complete, ought to have been ascertained. This was difficult in warm-blooded animals, a long continued heat exhausting them greatly, I therefore endeavoured to guess at this point. I generally waited till the inanimate bodies had acquired it, which was much easier, as I took care previous to the experiment to raise their temperature nearly to the degree which they would acquire after being placed in the stove. I here present the result of two experiments of this kind lately made.

I inclosed in the same basket, separating them only by a transparent partition, a rabbit and an alcarraza full of water. I placed them in a stove whose mean temperature during the experiment was 113 degrees. The temperature of the rabbit when introduced into the stove, was 103 degrees, that of the alcarraza about 95 degrees. That of the rabbit gradually rose to 110¾ that of the alcarraza on the contrary sunk to 86½ degrees, where it became stationary.

In the second experiment I exposed in the same stove at a mean temperature of 77¼ degrees, two small sponges and a frog. The frog placed between the two sponges acquired at the end of an hour a fixed temperature of 82½ degrees, the sponge on the left 82 degrees and the sponge on the right 81½ degrees.

the phenomenon, the object of our inquiry, shall cease, we may fairly conclude that it is owing to evaporation.

This mode of determining the influence of evaporation naturally presented itself to the minds of those who have investigated this subject. Some experiments have been made with this view; but they are neither numerous nor conclusive. One is by Dr. Fordyce. He caused a quantity of steam to be carried into a stove room, and found the heat more disagreeable, but the temperature of his body remained nearly stationary. It is however worthy of remark, that the time he remained in the stove room, was too short to heat in any sensible degree so large a mass as the human body. No positive conclusion, therefore, can be drawn from this experiment. Nor can greater advantage be obtained from that of Dr. Crawford, who endeavoured to ascertain the influence of a hot bath on the temperature of a dog, the method by which he measured the temperature not being exact; and besides the water would only suppress the evaporation from the skin, and not from the lungs. Similar experiments, that he made on frogs in which the evaporation from the lungs is very trifling, would be more conclusive, if the result he announced were confirmed by observation. This however I have found not to be the case. Repeated experiments very carefully conducted have proved to me that frogs uniformly acquire a temperature equal to that of the water in which they are immersed, whatever be its degree of heat, and that in this respect there is no difference between living and dead frogs.*

Such are the experiments, which to the best of my knowledge have been made with the view of determining what would occur in man and other animals exposed to a high degree of heat when no evaporation could take place from the surface of the body. They are evidently insufficient. New ones were required, which I have endeavoured to make.

For this purpose I had recourse to the means employed by Dr. Fordyce, but with this difference, that instead of trying the experiments on man, I selected animals of a small size, so

* See my memoir on the effects produced by great heat on the animal economy. Page 54, and subsequent.

that their bodies might be soon heated. These were well adapted to the end; for it is easy to perceive that if animals are placed in an atmosphere charged with vapor, there will be no evaporation of the fluids exhaled from the surface of the body, nor from the interior of the lungs, and yet their functions will be as freely performed as in a dry air.* The apparatus I used enabled me to distribute the vapors pretty uniformly through the whole space occupied by the animals, and to regulate the quantities at pleasure. I successively introduced different kinds of warm-blooded animals and frogs; I exposed them to different degrees of heat; I carefully examined their temperature, both before and after the experiment, by means of a thermometer, introduced into the rectum or the œsophagus. The result obtained will be found in the following table.†

* An idea will be readily formed of this apparatus by supposing a box of three feet four inches in height, four inches in width, and nearly the same in depth, divided into two chambers by a horizontal railed partition, placed about two thirds from the bottom. On one side of the box there is a door opening into the upper chamber. A little circular wicker work, placed in this chamber, forming a second inclosure in the chamber itself, with a door corresponding with that in the box. In this the animals are placed. A thermometer with a very long tube and small bulb, fixed in the centre of this inclosure, the scale being exterior to the box, indicates the temperature. It is secured from injury by a case of open wicker work.

The vapor is disengaged in a small tin kettle, whence it is conducted by a small bent tube opening into the bottom of the box. A square piece about an inch thick, placed above the bottom serves to break the current of vapor and to distribute it uniformly through the apparatus. Near the lower end of the tube of communication, there is a small stop cock so constructed that the vapour may pass by a lateral opening, by the tube itself, or by both at the same time. By these means the quantity of vapor in the box can be regulated at pleasure, and of course its temperature. This is facilitated by a very simple contrivance, which allows the person to turn the cock, without losing sight of the thermometer, which indicates the temperature of the box. The key of the cock is formed into a pretty long lever, at the ends of which are strings connecting them with the corresponding extremities of another lever of the same length. The latter lever turns on a pivot fixed to the case near the thermometer, and cannot be turned without exciting similar movements in the key of the stop cock.

† Some time ago I made similar experiments and communicated them in a memoir read before the Philomatic society, but not being made with sufficient exactness I declined publishing the memoir, and gave a short extract in the bulletins of that society.

TABLE*

Of the results obtained by exposing different animals to a moist heat, with the view of determining the influence of such heat on their temperature.

Number of the experiments.	Names of the animals.	The time of continuing in the box filled with vapor, expressed in minutes.	Mean temperature of the apparatus during the experiment, expressed in degrees of Fahrenheit.	Temperature of the animal after exposure to the vapor, expressed in degrees of Fahrenheit.	Temperature of the animal previous to its exposure to the vapor, expressed in degrees of Fahrenheit.
1	1st Rabbit	39′	101.6	108.3	104
2	1st Rabbit	55′	101.6	109.2	103.3
3	1st Rabbit	52′	105.2	110.5	104
4	2d Rabbit	55′	101.6	109.2	103.2
5	2d Rabbit	57′	101.6	108.8	104
6	2d Rabbit	55′	105.2	109.3	103.3
7	Guinea Pig	56′	99.9	108.8	102
8	2d ditto	55′	101.6	109.2	102
9	3d ditto	48′ 5″	105.2	110.3	102
10	4th ditto	55′	105.2	111.5	101.1
11	Pigeon	55′	99.9	110.8	108.5
12	Pigeon	40′	105.2	113	$107\frac{1}{4}$
13	Pigeon	42′	$107\frac{1}{4}$	116.4	$107\frac{1}{3}$
14	1st Frog	73′	78	79	
15	2d Frog	50′ *	81	82	

* I shall offer several remarks on the observations given in this table.

Besides the experiments whose results are here given, I have made several others on the accuracy of which I can rely. Their results have always been analogous to those contained in the table.

Whatever precaution I took there was always some variation in the temperature of the apparatus, during the experiment. These variations did not usually exceed two degrees, but occasionally for a very short time they extended to six degrees.

When any animal was subjected to several experiments, there was always an interval of twenty-four hours between each.

The thermometers used not moving uniformly, I carefully examined this difference, and prepared for each a scale, by which I could reduce to a common standard the different results furnished by these observations. Though in marking the results I have used the tenth of a degree, I do not pretend that the observations were always made with this nicety, which I preferred to committing any voluntary mistake. The errors cannot have exceeded more than the fourth part of a degree.

The temperature of the animal previous to its introduction into the apparatus frequently offered some slight differences, whose cause I never could ascertain.

It was not easy to determine the temperature of frogs immediately on being

In glancing over this table, we perceive that the temperature of warm-blooded animals, is uniformly raised four or six degrees at least above the moist atmosphere in which they are placed. It evidently follows that the power of producing cold had been destroyed, and that consequently this power essentially depends on evaporation. It is true that the heat, to which these animals were exposed, did not exceed their natural temperature more than two degrees; and it might be supposed that in a higher temperature, the power of producing cold would be more distinctly marked. But this objection to the conclusions I have just offered will vanish, when we consider that death would necessarily follow in these animals, on a longer exposure to a moist heat than I subjected them, and that consequently this power would have been extinguished. Indeed, small as may appear the heat which they sustained in these experiments, they were always more or less exhausted; and in those instances where it was greatest, they appeared dying on being withdrawn from the apparatus. The Guinea pig, though very lively in the morning, died the evening of experiment No. 10. A rabbit and a pigeon likewise perished after similar experiments, whose results have not been given in the table.

The question may perhaps be asked, why the temperature of the animals did not acquire an exact equilibrium with that of the surrounding medium, and why did it rise a few degrees higher. The answer to this question is very plain. Their functions not being disturbed, the cause, whatever it may be, that produces animal heat, continued to act, and occasioned the elevation of their temperature. It is more difficult to understand why this elevation was not greater: why the same cause which in low degrees of temperature maintains the animal at 40, 80, or even 160 degrees above the temperature of the surrounding

removed from the box, and without its being affected by contact of the hands or external air. To accomplish it I placed the animal on a small carriage, with the thermometer having a very small bulb resting in the mouth or rather in the stomach. On opening the box, the carriage was rapidly withdrawn, and the degree examined as indicated by the thermometer.

The animals exposed to these experiments were more or less exhausted. When the heat was much increased, they appeared dying at the end of the experiment. The guinea pig was dead at the end of the experiment, No. 10.

medium, should only raise it six or eight degrees, when they are exposed to heat.* This difficulty cannot be resolved till we are able to give a satisfactory answer to the important and frequently disputed question, *what is the cause of animal heat?* A question, according to the facts I have stated, not at all connected with the subject of the present memoir.

The difference between the temperature of frogs and of the surrounding medium, (I believe the same would be true in all cold-blooded animals) has been less obvious than in warm-blooded animals, as might naturally be expected. This led to a very curious remark, which however requires to be confirmed by repeated experiments; namely that the heat of these animals, or the excess of their temperature above that of the surrounding medium, is as considerable when they are exposed to heat, as when they are exposed to cold. Whence it might be inferred that the cause of heat is not the same in them as in warm-blooded animals.

From all that has been stated, it follows that the refrigeration manifested in animals exposed to a great heat ought to be ranked among those phenomena whose cause is essentially physical. It cannot however be denied that the influence of the vital powers, as I have asserted in the beginning of the memoir, concur with the physical in producing all the phenomena which proceed from organization. For, in order that the evaporation producing the cold may take place, it is requisite that the surface of the body and the partition of the pulmonary cells should be constantly moist. But here the comparison between inorganic bodies, and those which were the subject of my experiments ceases. The sides of the former are moistened by simple transudation. In animals they are moistened by perspiration, a function extremely complicated and necessarily dependent on the action of the capillary vessels. In unorganized bodies, when the sides begin to dry, they

* Some new inquiries, which I have made since this memoir was read before the institution, and which I shall shortly publish, induced me to believe that the evaporation was not completely prevented in the experiments just related; but these results do not in any degree invalidate the conclusions I have drawn; on the contrary they rather tend to confirm them.

attract fresh moisture from within: but in animals the perspiration must acquire greater activity, when the heat is raised; and this can only be effected by an increased energy of the exhalent system, or perhaps of the whole circulation. It ought to be remarked that this invigorated activity of the perspiration at the surface of the body, at least is much greater than is necessary to supply the increase of evaporation: hence the sweat, which is nothing more than an excess in the perspired fluids beyond what is carried off by evaporation.

I shall close this memoir by offering the following proposition, which I think I may advance as a necessary corollary from the observations which I have related. The production of cold manifested in animals exposed to a high degree of heat arises from the evaporation of the matter of perspiration, which in proportion to the increased activity of the exhalent system is more considerable as the heat is greater. Hence it is at once the effect of vital and physical causes.

On the Gelatine of the Blood.

BY JOHN BOSTOCK, M. D.
OF LIVERPOOL.

From the Medico-Chirurgical Transactions, Vol. I.

Although different opinions have been entertained respecting the purposes which the blood serves in the animal economy, yet its obvious influence over the functions of life, has, at all times, rendered it an important object of research, both to the physiologist, and the chemist. A complete investigation of its composition and properties is, however, attended with much difficulty: it is a fluid consisting of several ingredients, possessed of qualities peculiar to themselves, and existing in a state of combination, of which we have no other example. According to the latest and most accurate analysis of this fluid, it is supposed to consist of the following parts: fibrine, albumen, gelatine, red globules, soda, some neutral and

earthy salts, a small portion of sulphur, and a peculiar phosphate of iron, all held in solution by a large quantity of water.*

The union of the fibrine and the red globules composes the basis of the crassamentum or clot, which spontaneously forms in the blood, shortly after its evacuation from the vessels. It is to the red globules that the iron is attached; and it seems probable, that from this metallic impregnation, their peculiar colour, and of course, that of the whole mass of blood, is derived. The albumen, the jelly, and the different salts, all dissolved in water, constitute the serum. The characteristic property which the albumen possesses, of being concreted by heat, affords an easy method of obtaining it in a separate state; if, after being rendered insoluble by the process of coagulation, it be cut in small pieces and digested in water, the other ingredients of the serum will remain suspended by the fluid, while the albumen itself is left behind in a state of considerable purity. By evaporating the water we obtain the jelly; but it is unavoidably mixed with the salts, from which it does not appear possible entirely to separate it; this object can only be in part accomplished, by a slow evaporation of the water, in consequence of which, a portion of the salts will assume their regular crystalline form, and may be thus removed from the mass. The small quantity of sulphur, which exists in the blood, appears to be united to the albumen; it has, however, never been obtained in a separate form, and its existence must be regarded as somewhat problematical. That part of the serum, which remains fluid, after the albumen has been coagulated by heat, to which the name of *jelly* or *gelatine* has been applied, is the last of the constituents of the blood, the presence of which has been distinctly ascertained; and it is the one to which in the present paper I propose principally to direct my attention.

In order to give a complete account of the subject, it will be necessary to review the different opinions that have been successively entertained respecting the constitution of the blood

* Parmentier & Deyeux, Jour. Phys. T. xliv. p. 438, 9.
Birkbeck de sanguine, tent. inaug.
Thomson's Fourcroy, V. iii. p. 270, 3.
Fourcroy, Système, T. ix. p. 140.
Thomson's Chemistry, iv. 585 & seq.
Delametherie, sur les étres, &c. T. ii. p. 148.

in general, the denominations that have been given to its different parts, and the state of relative combination in which they have been supposed to exist. In this review, I shall not entirely confine myself to those authors who have devoted their attention expressly to this subject; I shall notice the opinions of some writers who have only incidentally mentioned it, as from such sources we are often able to ascertain with equal correctness the progressive changes which take place in our knowledge upon topics of this description. After having accomplished this object, in as brief a manner as is consistent with accuracy, I shall give an account of some experiments that I have performed on this part of the blood, which have induced me to adopt an opinion respecting it different from that generally adopted.

We need not be long detained by the opinions of the ancients, on this subject. Although they were frequently accurate in their descriptions of the phenomena of disease, and have manifested considerable diligence in anatomical investigations; of chemistry they were entirely ignorant, and their physiology was so much perverted by preconceived hypotheses, as to have deservedly fallen into complete neglect. According to the opinion of Galen, which, like the other opinions of this celebrated man, was implicitly adopted for some centuries, the blood consists of four parts: blood, properly so called, phlegm, bile, and black bile. Under the first denomination he appears to have included the compound of fibrine and red globules, which we call the crassamentum; the phlegm was used to designate the serum; and the title of black bile appears to have been given to the red globules at the lower part of the clot, which, in consequence of being secluded from the air, had acquired a dark hue. It is not easy to discover to which of the ingredients of the blood the name of bile was attached.*

The illustrious Harvey, who so successfully established the true hypothesis respecting the motion of the blood, appears to have been the first of the moderns who entertained any just

* Galen, de elem. lib. 2. Harvey, de gen. Exer. 52.
Willisii Opera, T. i. p. 66.
Boerhaavii præl. ab Hallero, T. ii. p. 329.
Castelli Lexicon, hæma, arrhus, ichor, phlegma, &c.

conception of the nature of its composition. In his treatise on generation, published in the year 1651, he distinctly speaks of its separation into fibrous and serous parts, as effected by the process of spontaneous coagulation. At the same time he describes a third substance, which occupies the higher part of the clot, and resembles jelly, mucilage, or albumen ovi, to which he gives the title of mucago, and which he regards as the part of the blood the most abounding with spirit. Some authors have supposed, that by this substance he meant to describe the buffy coat, which occasionally appears on the surface of the blood, and others, the substance resembling cream, which sometimes floats on the serum, and was particularly attended to by Hewson and Hunter; but these opinions seem to be scarcely tenable, as this mucilaginous covering is described rather as a constant than as an accidental occurrence. Harvey is supposed to have been the discoverer of the property which the albumen possesses of being coagulated by heat,* and he says, that, by this process, the serum is converted into the peculiar mucilage described above. His claim is rested upon the following passage.—" Et quemadmodum, crudescente san-" guine, parum admodum istiusmodi mucaginis supernatantis " reperitur; ita, si saniem illam a grumo separatam, et effusam " leni foco decoxeris; eandem brevi in mucaginem hanc muta-" tum iri conspicies."—" Quippe urina coctione non densatur " in fibrosum mucaginem, sed potius in lixivium: aquosa au-" tem, sive saniosa hæc pars (sanguinis) aliquandiu leviter " cocta, in mucaginem, innatantem abit:—"†

It must be confessed, that this is not a very accurate description of the process, nor does the term mucago seem at all applicable to the coagulated serum. We may perhaps explain the passage, by supposing, that the coagulation was imperfectly performed, in consequence of the heat either not being sufficiently raised, or not having been long enough applied.

A more accurate idea of the coagulable nature of the serum was entertained by Lower, who, in his treatise on the heart,

* Haller, El. Phys. lib. v. sect 3. § 2.
Thomson's Chem. v. iv. p. 587.
† Harveii Exer. lii. de gen.

published in 1669, describes the liquor pericardii as being similar to the blood, because by the application of heat, it was coagulated and converted into a white jelly.* The fact is mentioned in equally distinct terms by Willis, whose works were published shortly after his death, which took place in 1675. He supposed the blood to contain spirits, sulphur (from which its red colour is derived), salts, earthy matters, and waters; and after a good deal of whimsical hypothesis about the analogy of the blood, first to wine, and afterwards to milk, he notices its division into the red fibrous part, and the coagulated serum. If this latter, he informs us, be exposed to the heat of a fire, it is concreted like the white of the egg, and a coagulum is likewise thrown down from it, by the addition of an acid.†

A considerable advance in our knowledge, respecting the nature of the crassamentum, was made by Malpighi. In his treatise on polypi of the heart, probably written between 1670 and 1680, he combats the idea that was generally entertained, that they consisted of coagulated serum, and gives it as his opinion, that they are formed from the crassamentum of the blood. He informs us, that by repeatedly washing the clot, its colouring matter is entirely removed, and a white substance is left, which, when viewed through a microscope, exhibits a reticulated or fibrous structure, exactly resembling that of polypi. He supposes the buffy coat, ("pellea crusta") which occasionally appears on the top of the clot, to be formed of the same fibrous substance.‡

The knowledge which Borelli possessed respecting the composition of the blood was still farther matured. The albumen and the cruor, the parts into which the blood spontaneously separates, he conceived were themselves compounded bodies, the albumen consisting of a substance coagulable by heat, exactly like the white of the egg, and a watery serum impregnated with salts, while the clot was supposed to consist of a very glutinous substance, united to a purple juice. By repeated

* Lower de corde, p. 6.

† Willisii op. T. 1. p. 72.

‡ Malpighi op. de polypo cordis.

washing, the clot is converted into white fibres or reticulated membranes, while the red matter is carried off by the water.*

The celebrated Boyle wrote his 'Natural History of the Blood' in 1683, two years after the publication of Borelli's work; but although a very elaborate performance, and of considerable length, it cannot be said to have added much to our knowledge respecting the nature of the blood. The attention of the author is almost exclusively occupied with an account of the effects of different chemical reagents upon it, and he relates a number of processes which he performed for the purpose of obtaining, what he calls the spirit of the blood. He speaks of its division into the fibrous and serous parts, and correctly notices the effects of heat, acids, alkalies, alcohol, and the oxymuriate of mercury, in coagulating the serum.

The chemical analysis of the animal fluid was, at this time, pursued with great ardor; but as fire was the principal agent employed, it is not to be supposed that much important information would be obtained. Juncker is supposed to have been the first who analyzed the blood by destructive distillation; he resolved it into "water, spirit, volatile salt, oil, and fixed oil:"† except the water, these were all new products formed during the process. It was about this period that Leeuwenhoek entered upon his laborious microscopical researches into the nature of animal fluids, and among other curious discoveries, detected the presence of the red globules of the blood. He first announced this discovery in a letter to the secretary of the Royal Society, dated August 15th, 1673. Although he appears afterwards to have gone into much unfounded speculation respecting the composition of these globules, their existence has been since amply confirmed, and the discovery must be considered as forming an important step in the progress of our knowledge.‡

It had been long known that the blood contains a saline impregnation; but the first attempt to ascertain the nature of the salts, appears to have been made by Gulielmini, who, by slowly evaporating serum upon glass, obtained them in a crystallized state, and was thus able to distinguish their figure; he acknow-

* Borelli de Motu Anim. T. ii. prop. 132.

† Junckeri, Chymia, p. 75. ‡ Phil. Trans. for 1674, p. 23.

ledges that this method was first pointed out to him by Malpighi.* Gulielmini also paid a good deal of attention to the coagulable lymph, to which he gave the name of "fibra sanguinis." He wrote his treatise on the blood in 1701. Lancisi's learned dissertation on the motion of the heart and arteries was also written early in the 18th century, and contains many observations on the nature of the blood. He directed his attention particularly to the red globules, and controverted the hypothesis of Leeuwenhoek, who conceived that each globule was composed of a series of smaller globules.†

The constitution of the blood formed a very important part of the theory of the celebrated Boerhaave, and he consequently paid much attention to its nature and properties; yet it does not appear that he materially added to the knowledge which was previously acquired upon the subject, and indeed, in some particulars, he seems to have had less accurate ideas respecting it than his contemporaries. Leeuwenhoek's hypothesis of the progressive series of globules, was adopted by Boerhaave in its fullest extent, and formed a fundamental part of his pathological doctrines. From some passages in his writings it may be inferred, that he did not consider the serous and fibrous parts of the blood as essentially distinct from each other, nor from the red globules, but that they all consisted of the same matter differently arranged, and he even speaks of the different parts as being converted occcasionally into each other.‡ Boerhaave entered upon the discussion of the question, which was at one time much agitated, whether the blood was acid or alkaline, and from finding that no effervescence was produced by the addition of either acids or alkalies to it, he concluded that it possessed neither of these qualities.§ It is, however, now known that this conclusion was erroneous, and we are able to perceive the source of the error into which he was betrayed.

We may notice the account that Hoffman gives of the blood,

* Guliemini Opera. T. ii. sect. 44.

† Lancisi, Opera. T. iv. de motu cord. post xvi.

‡ Boerhaave, aphor. sect. 93, 94.

———, prælect. sect. 223 & not.

§ Boerhaave, chem. T. ii. proc. 114.

rather in consideration of his general celebrity, than from any peculiar sagacity which he displayed upon this subject. Blood, he supposes, is composed of watery, sulphureous, and earthy parts, that it is nothing more than a purple jelly, although it differs from common jelly in being more inflammable, in yielding more volatile salt and oil when distilled, and in being more fetid when it putrefies. He concludes therefore that blood is a jelly of a more concocted or subtile kind.*

The next account of the blood that we meet with is in Senac's elaborate dissertation on the heart. He describes in detail its different constituents, with considerable minuteness; beginning with the red globules, he afterwards proceeds to what he calls the lymph, then the gelatinous matter, the mucus, and lastly the serosity. By lymph he certainly meant to designate the part which we now call fibrine; he, however, appears to have had no very accurate conception of the difference between the fibrine and the albumen, as he says the lymph may be hardened by exposure to heat; and he also supposes that the buffy coat of inflamed blood is composed o f albumen. With respect to the jelly and the mucus said to be in the blood, he seems rather to have inferred their existence from analogy, than to have actually detected their presence. He compares the jelly of the blood to that which is found in broth, and says, that it may be obtained from the blood by boiling it. The existence of mucus he infers, from the quantity of this substance which is poured out on the secreting surfaces with which the body abounds; conceiving it more probable that it should have been ready formed in the blood, than generated by glands lying contiguous to the parts. By the serosity Senac unquestionably meant the serum; he says it is coagulated by heat, and resembles the white of the egg; and he points out the particulars in which it differs from jelly.† As far as I have been able to collect, Senac is the first writer who employed the words coagulable lymph and serosity; he used the first in the same sense in which we now employ it; but he certainly considered serosity as synonymous with serum, and he does not appear to have had

* Hoffman, Rat. Med. lib. i. sect. 1. cap. 5.
† Senac traité du cœur, liv. 3. chap. 4.

any idea of the existence of that part of the blood to which some late writers have applied the term.

Not long after Senac, De Haen published some remarks upon the blood. He points out the method of obtaining the fibrine, or as he styles it, the membrane of the blood, by rapidly stirring it, when fresh drawn, with a stick (a process which he informs us was first practised by Ruysch) or still more readily, by agitating the blood in a bottle. After noticing the resemblance of the serum to the white of the egg, he informs us, that if blood be suffered to flow into warm water, a substance is procured from it which he calls "gelatum." This was probably a thin film of coagulated fibrine; it certainly could not have been what has since been called the jelly of the blood.

As M. Fourcroy refers to De Haen, as the original observer of the gelatine, I shall quote at length the passage in which he describes this peculiar substance.

"Sanguinem alium ex brachio, alium ex pede effluentem, in "aquam ad 100 gradus thermometro imposito calentem, ex- "cepi. Sanguis ille observatur primo hanc acquam æquabili "rubedine tingere, si jactus ejus citatus est: refrigeratæ dein "aquæ innatat plurimum albescentis pellucidi, glutinosi, fugi- "entis ad tactum, manu, aut cochleare vix capiundi; hæc si "brevi a venæ sectione rem explores. Si vero per 8, 10 horas "aqua illa post venæ sectionem sine examine reponatur, revo- "ceturque ad examen, habebit nonnunquam parum illius pellu- "cidi glutinosi, sed multo solidius, colore fuscum, facilius capi- "endum. Hoc contentum quo scirem quid esset, pluries illud "charta emporetica, linteo suffulta, percolavi: nonnunquam "remansit nihil; aliquoties quid pauci, glutinosi, quod exsicca- "tum furfura, griseive pulveris instar, in spiritu vini perstitit. "Materies hæc glutinosa, ex aqua adhuc calente, mox a venæ "sectione exceptæ, et vel ramulo, aut lagena, agitata, iteratis "experimentis nihil unquam dedit membranosi. Si experi- "mentum instituis longè post venæ sectionem, ubi aliquoties "contenta materies solidior apparet, est tamen idem experimenti "effectus. Hæç porro solidior materies, ab ea, quæ ex calente "adhuc aqua eximitur, differt in eo, quod percolata plus relin- "quat materiæ glutinosæ fuscæ. Hanc superstitem a percola-

"tione materiem per horam agitavi lagena, visurus num mem-"branam daret? Nequaquam; agitatio illa materiem modo ex "fusco reddidit rubicundiorem. Percolata omnium horum ex-"perimentorum aqua rubella est, et mucosi quid in fundo "habet. Bidui spatio omne id mucosum fere evanescit in aqua. "Experimenta hæc sæpius, & in phlogistico sanguine, & in "sano, capta sunt. Gelatum ergo illud, quod in aqua calida a "misso sanguine colligitur, neque materies factitiæ membranæ "est, ut mox ostendi, neque etiam est crustæ phlogisticæ ma-"teria. Est enim gelatum hoc in quavis aqua, in quam calen-"tem sanguis sive ex brachio, influxerit. Saltem si esset "crustæ materies, ii, quibus neque a morbo, neque a gravitate, "neque a diathesi crusta adesset, gelati carerent."*

Our knowledge respecting the blood was in this state, when in the year 1759, the celebrated Haller published the second volume of his Elements of Physiology, in which he treats with considerable minuteness, of the nature and properties of the blood. He details the opinions of many of his predecessors, and discusses the various controverted points with much of his accustomed candor and learning. We cannot, however, but remark, that this illustrious physiologist entertained less correct notions about the blood, than might have been expected from one possessed of his extensive information and indefatigable research. After noticing the separation into crassamentum, or as he calls it, cruor, and serum, and describing the former of these substances, he devotes a section to the particular examination of the latter. He supposes that it consists of water, mucus, and jelly; but it appears evident from several parts of his work that he did not employ the term jelly in that confined sense to which it is restricted by the accuracy of modern chemistry. He seems not to have been aware of the distinction which exists between the albumen of the blood, and the jelly which is obtained by boiling the membranous parts of animals; he also confounds it with the exudation which proceeds from wounded vessels, and which closes up the mouths of arteries, and he even imagined that polypous concretions, and the buffy coat of inflamed blood are formed of albu-

* De Haen, Rat. Med. pars 1. cap. 6.

men. Hence we may safely conclude, that, although he enumerates jelly among the constituents of the serum, he was not acquainted with the substance to which the term gelatine has been since applied by Fourcroy and other later writers; there is no evidence of his having obtained it in a separate state, or even having been aware of its existence; and he evidently uses the word jelly to describe a substance of very different properties. By the mucus of the blood, Haller meant to speak of that substance which was obtained by De Haen, by permitting the blood to flow into warm water; for the proof of its existence, he refers to the passage which I have quoted above from this author. I do not find that the word serosity is mentioned by Haller in his Elements of Physiology.*

From the account which I have given of the authors who have treated of the subject before this period, we may conclude, that the existence of any animal matter in the serum, distinct from the albumen, and not coagulable by heat, was unknown to them; much less was there any idea entertained of the serum containing a proper jelly. When this word was employed by these writers, to designate any of the constituents of the blood, they used it in a vague sense, generally applying it to the serum, but sometimes to the whole mass of the blood. We may remark that the word jellying, or some synonymous term, is frequently employed to describe the process of coagulation, both the spontaneous concretion of the crassamentum, which takes place when blood is drawn from the vessels, and the consolidation of the albumen, which is effected by heat. Of this we have the most striking illustration in the works of Haller himself, who intitles one of his sections, "Seri pars quæ cogitur, vel gelatinosa."

The publication of Haller's great work may justly be considered as forming an important era in the history of the science; by rendering information more accessible, it tended materially to the diffusion of physiological knowledge, and thus immediately prepared the way for many important improvements. After having ascertained the state of the opinion that was en-

* Haller, El. Phys. lib. 5. sect. 3.

tertained respecting the serum of blood at this period, I shall now proceed to trace the different changes which it has undergone until the present day.

The first clear notification which I have met with, of an uncoagulable animal matter in the serum, is in an inaugural dissertation published at Edinburgh in 1760, by Dr. Butt. After remarking the separation of the blood into serum, and crassamentum, he observes, that serum consists of two parts, a coagulable substance, and a watery fluid, which last also exhibits marks of containing some animal matter. Dr. Butt fell into the inaccuracy, which was common among his contemporaries, of considering the albumen and coagulable lymph not to be essentially different from each other; he expressly states, that he regards the white of the egg as nothing more than coagulable lymph in its purest form.* No notice is taken of this peculiar part of the serum by Gaubius, who published his pathology in 1763; indeed he so little understood the nature of this fluid, as to conclude that its glutinous texture depended upon a mucilaginous matter, resembling vegetable gum.†

It was three years after this period, that the celebrated Cullen first gave medical lectures at Edinburgh; and I believe, it was about the same time, that he published, for the use of his class, the text book intitled "Institutions of Medicine," in which we may observe some farther advances in our knowledge respecting the constitution of the blood, although the account which he gives is still embarrassed with much inaccuracy. What had been called the fibrous part of the blood by Malpighi, Gulielmini, Gaubius, and others, and by Senac the coagulable lymph, Cullen denominated gluten. He observes, that when the serum has been coagulated by heat, if it be cut into small pieces, a thin fluid, of a saline taste, exudes from it; to this fluid he gave the name of serosity; and he considered the serum as formed merely by the solution of a quantity of gluten in this serosity.‡ Hence it appears, that he had not a distinct conception of the difference between the albumen and the gluten, nor of their re-

* Butt, p. 53. & alibi.

† Gaubii Inst. path. sect. 337, & seq.

‡ Cullen's Instit. of Med. sect. 247. & seq.

lation to each other. We may remark also, that the sense in which he employed the word serosity, was very different from that given to it by its original inventor Senac; yet the wide diffusion which the opinions of Cullen would acquire, in consequence of his situation as a public teacher, necessarily gave them a great degree of currency; and we accordingly find that, from this period, the word serosity was generally restricted to the uncoagulable part of the serum.

In the year 1770, Mr. Hewson first published the result of his inquiries into the nature and constitution of the blood; some of his opinions, particularly those respecting the red particles, are now discarded, as being without foundation; but on many points he will be found to be considerably more correct than any of his predecessors. After remarking that the coagulable lymph and serum had been confounded with each other, even by the best informed writers, immediately preceding the period when he wrote, he proceeds to notice their distinctive characters, and after describing the coagulation of the latter substance, observes that a watery fluid may be pressed out of it, which he calls the serosity. This fluid, he says, contains the neutral salts of the blood, and also a mucilage which cannot be coagulated by heat; but if part of the water be evaporated, it then acquires a firm consistence, and resembles the mucus spit up from the lungs, when dried.* Mr. Hewson has indeed fallen into an error, in supposing that this peculiar fluid has the name of serosity given to it by Senac; this author, as we remarked above, seems to have been the first who used the word, but he applied it to the serum at large, while Cullen restricted it to this particular part of it. Gaber, who performed his experiments on pus about the time of Mr. Hewson's publication, and who imagines that purulent matter was formed from what he calls serosity, evidently intended, under this denomination, to speak of the serum at large.† Dr. Gregory, on the other hand, in his "Conspectus," uses the word serosity in the same sense with Cullen.‡ I do not find that this part of the blood is noticed by Mr. Hey in his "Inquiry."

* Phil. Trans. 1770; & Inquiry, passim.
† Journ. de Phys. Introd. T. ii. p. 23.
‡ Gregory, Conspect. T. i. sect. 592.

No particular alteration appears to have taken place in the opinions respecting the composition of the blood until the year 1790, when the following discovery was announced to the academy of sciences by MM. Fourcroy and Vauquelin. "If "serum be exposed to heat, after being mixed with half its "weight of water, it is in part coagulated; and the portion of "liquid which is not coagulated contains gelatine, which gelatinizes by cooling."* The discovery was developed more at large in a future paper, in which the authors inform us, that a slightly turbid fluid may be separated from coagulated albumen, which, by evaporation and cooling, concretes into a substance possessing every characteristic of true jelly.† They state that this substance had been seen by De Haen, though without referring to any particular part of his treatise; but from a careful perusal of it, in connexion with the references made to it by Senac and Haller, I believe there can be no doubt, that it is the passage which I have quoted above that contains the supposed discovery of De Haen. I have already stated, that I am disposed to draw a different conclusion from it.

A still more particular account of this substance is contained in a paper published by MM. Parmentier and Deyeux in 1794. They appear at first to have entertained some doubt about the accuracy of M. Fourcroy's conclusion, and took considerable pains to ascertain the properties of this supposed gelatine. Their experiments led them to be fully satisfied as to its existence; and they relate with minuteness, both the process which they employed to procure it, and the nature of the substance which they obtained. The properties, however, which they ascribe to it are neither essential to, nor characteristic of, jelly. They do not state that it is capable of being concreted by cold, nor do they notice the effects of any decisive chemical tests; they inform us that it is glutinous to the touch; and that when dried, it composes a hard transparent film.‡

* Ann. de Chimie, T. vi. p. 182.
† Mem. Acad. Scien. 1789, p. 297.
Ann. de Chimie. T. vii. p. 146.
‡ Journ. de Phys. T. xliv. p. 438, 9.

At the same time, that the French chemists were thus occupying themselves with detecting the properties of this ingredient in the blood, the celebrated John Hunter, ignorant probably of their operations, was likewise engaged in examining the same substance. He describes the serum of the blood as consisting of two distinct fluids, one coagulable by heat, like the white of the egg, and the other, which remains uncoagulated. He discovered that this peculiar part of the serum is precipitated by Goulard's extract; and in this manner he attempted to ascertain the relative proportion in which this substance and the albumen of the blood existed in the blood of different individuals, and in other serous fluids which he examined. This method, however, as will afterwards appear, is totally inadequate to the purpose. None of the properties of this substance, as pointed out by Mr. Hunter, are at all analogous to those of jelly, nor does it appear that he, in any respect, considered it as a gelatinous fluid.*

Since this period no material change seems to have taken place in the opinion entertained respecting the constitution of the blood, at least with regard to the existence and properties of the gelatine. Those writers who have treated the subject the most copiously, Dr. Birkbeck,† Mr. Allen,‡ M. Dumas,‖ M. Fourcroy,§ Dr. Thomson,¶ and M. Delametherie,** agree in representing one of the constituents of the blood to be a proper jelly, which is liquified by heat, and congeals again by cooling; and the same opinion is maintained by Professor Blumenbach,†† Mr. Hatchet,‡‡ and M. Richerand,§§ who incidentally mention the subject. The only author of respectability, who supports a different doctrine, is Mr. John Bell; but I conceive that a perusal of his remarks on the blood will prove, that

* Hunter on the Blood, p. 32, 3.
† Tent. Inaug. de sanguine, passim.
‡ Thomson's Fourcroy, T. iii. p. 270—3.
‖ Dumas, princip. de phys. T. ii. p. 37, 8.
§ Fourcroy, systeme, T. ix. p. 140.
¶ System of Chemistry, V. iv. p. 585. & seq.
** Delametherie, Considerations, T. ii. p. 148.
†† Blumenbach, Inst. Phys. sect. 11.
‡‡ Phil. Trans. 1800. p. 401. §§ Richerand, Elem. Phys. p. 184.

notwithstanding his acknowledged talents, and the acuteness with which he has detected the mistakes of others, he has himself, on this subject, betrayed a great deficiency of information. He says that blood consists of crassamentum, serum, and red globules, forgetting that the red globules are one of the component parts of the crassamentum; he uses the terms gluten and jelly as synonymous, and expressly states, that all attempts to distinguish between the glutinous and albuminous parts of the blood are vain and useless. Although he frequently employs the word jelly, when speaking of the constituents of the blood, it does not appear that he had any distinct conception of that part of the serum which we are now describing; indeed the whole section is so confused, that it is not easy to develop the author's exact meaning.*

Having thus completed a sketch of the opinions that have been entertained by others upon the subject, it remains for me to give an account of my own, and especially to point out the circumstances in which they differ from those of my contemporaries. Before, however, I can do this, I shall be under the necessity of adverting to some experiments that I have lately made on the analysis of animal fluids, and on the method of discriminating between those which the most nearly resemble each other in their visible properties, and which, in consequence of their being found in greater or less proportion in almost all parts of the body, I have denominated primary. Of these there are three, albumen, jelly, and mucus. The distinguishing characters of the first are its being coagulable by heat, and by the oxymuriate of mercury. The second is liquified by heat, and becomes solid again by cold: it is not affected by the oxymuriate of mercury, but is precipitated from its solution by tan; while mucus is neither coagulated by heat, nor has the power of concreting by cold, it is not affected by the oxymuriate of mercury, nor by tan; but it is copiously precipitated by the acetate of lead.†

In order to ascertain the nature of the uncoagulable part of the serum, I exposed a quantity of it for some time to the heat

* Anatomy, V. ii. p. 87, & seq.

† Ed. Med. Journ. V. i. p. 257. Nicholson's Journ. V. xi. p. 244.

of boiling water; it concreted, in the usual manner, into a solid mass; but upon being divided into small pieces, and laid upon an inclined pane of glass, a brownish liquor oozed from it. The pieces of serum were afterwards digested in boiling water, which became tinged of a brown colour, owing to some substance previously contained in the serum which it had carried off. The fluid which oozed from the coagulated serum, and the water in which it had been digested, were added together. To a portion of it a small quantity of the solution of the oxymuriate of mercury being added, it became milky, and a precipitate was formed; it was also rendered opake by being for some time exposed to the boiling temperature. Hence I found that it still contained some uncoagulated albumen; and in order more effectually to separate it, I diluted a quantity of serum with six times its bulk of water; to this I added the solution of the oxymuriate of mercury, until no farther precipitation could be perceived, and placed the compound in the water bath. The coagulum was by this process rendered considerably firmer than when heat only had been employed, and the liquor remained nearly transparent; it was passed through a filtre, and now no precipitate could be obtained by the addition of the infusion of tan. A quantity of the water in which coagulated serum had been digested was slowly evaporated; when the greatest part of the water was separated, it was suffered to cool, but no appearance of gelatinization was perceptible. The evaporation was then continued to dryness; a tenacious film of animal matter was left behind, which did not in any respect resemble dried jelly, and which was with difficulty redissolved by the addition of more water. These experiments were several times repeated; and the results were essentially the same so far at least as affected the conclusion to be drawn from them. It is necessary however to remark, that in trying different specimens of serum, there was a considerable difference perceptible in the readiness with which the albumen was separated from the uncoagulable part; in some instances a single operation was sufficient, while in others it was necessary to repeat the addition of the oxymuriate of mercury and the boiling four or five times, until the liquor which was passed through the filtre was entirely freed from the uncoagulated albumen.

From these experiments I felt myself justified in concluding:—First, That when diluted serum is completely deprived of albumen, which is proved by its no longer yielding a precipitate, upon being boiled with the oxymuriate of mercury, it is not affected by the infusion of tan. Secondly, That the animal matter contained in serum, which is not coagulated by the operation of heat or the oxymuriate of mercury, does not possess the property of concreting by cold. Whence we may infer, in the third place, That that part of the serum which is not coagulable by heat does not possess the properties which are essential to jelly, either physical or chemical.

Having thus found that the uncoagulable part of the serum is not jelly, I was induced, from my ideas of the constitution of animal fluids, to consider it as consisting of mucus. I have not indeed been able unequivocally to establish this opinion; for although the water, in which the albumen had been digested, was very copiously precipitated by the acetate of lead, yet it might be conceived that this effect was produced by the decomposition of the different saline bodies that exist in the blood. I however thought myself authorized in considering it to be a proper mucus, both from the nature of the precipitate produced by the acetate of lead, which exhibited the peculiar flaky form, which is indicative of an animal or vegetable impregnation, and likewise from observing the appearance that it assumed by evaporation. Before I leave this part of the subject I may observe, as a confirmation of the inferences deduced from my experiments on serum, that I have had an opportunity of examining the fluids from a tumor on a diseased spine, from a case of hydrocephalus internus, and from a hydrocele, and that I could not in any of them detect the least trace of jelly.

As the opinion which is entertained, by authors of the first respectability, respecting the uncoagulable part of the serum, appears to be incorrect, it may be thought incumbent upon me to point out the circumstances which have contributed to establish the erroneous doctrine. In the first place we may observe, that a considerable degree of inaccuracy pervades the language of even the most correct writers on subjects connected with

animal chemistry. The terms jellying or gelatinization, which ought to be restricted to the property that heated jelly possesses of becoming solid by cold, have been applied to every case in which a fluid substance is converted into the concrete state; whether by heat, as in the instance of the albumen, or by what has been called spontaneous coagulation, as is observed in the fibrine. This inaccuracy may probably, in the first instance, have misled M. Fourcroy, who finding these expressions employed by writers of high reputation, as applied to the constituents of the blood, entered upon his experiments with his mind biassed in favour of the idea, that he must meet with jelly as one of its component parts. When, under this impression, he instituted his experiments upon the uncoagulable part of the serum, it is easy to conceive, that he might mistake the effects of desiccation for those of gelatinization; and as far as appears, he did not employ any other method of ascertaining the nature of the substance, than the change produced in it by the effect of heat. Indeed, although he states the fact in different parts of his works with perfect confidence, and claims the discovery, as one which he thought of importance, he no where gives us any account of the manner in which his experiments were conducted. The subject was treated much more in detail by MM. Parmentier and Deyeux; but although they conclude in favour of M. Fourcroy's opinion, we shall not find that their results authorize this conclusion. They do not say that the substance, which they obtained, concreted by cold; nor do they seem to have thought of examining its nature by any chemical tests; the properties which they point out are by no means characteristic of jelly.

As to Mr. Hunter's experiments, it is obvious that they rather confirm, than oppose, the opinion that I am desirous of establishing. He found the part of the serum, which is not coagulable by heat, to be precipitable by the acetate of lead, a substance, which is the appropriate test of mucus, and has no action upon jelly. He does not appear to have examined the effects of heat upon it. The authors who have more recently treated upon the subject appear, at least for the most part, to have taken up the opinion of the French chemists without

farther examination; and indeed, after the very direct manner in which it had been stated, we can scarcely accuse them of rashness or improper confidence. Both Dr. Birkbeck and Mr. Allen, however, mention the effect of tan in throwing down a copious precipitate from the serosity; but from the way in which it was obtained, by pressing it from serum that had been exposed to heat, we must conclude, that it still contained a portion of uncoagulated albumen, which would be acted upon by the tan. Thus it appears that the erroneous opinion, which these gentlemen derived, in the first instance, from the French chemists, was confirmed by the circumstance of their operating upon the substance in an impure state.

On the Nature of Oximuriatic Acid,

IN REPLY TO MR. JOHN DAVY. BY J. MURRAY, LECTURER ON CHEMISTRY, EDINBURGH.

From Nicholson's Journal, for April, 1811.

To Mr. NICHOLSON.

SIR, Edinburgh, March 11, 1811.

I SHALL beg your permission to occupy a few pages of your Journal with some remarks on the reply of Mr. J. Davy, in your last number, to the observations which I had offered on Mr. Davy's opinion of the nature of muriatic and oximuriatic acids.

The gentleman first remarks, that I have not attended to the distinction between theory and hypothesis, but have made promiscuous use of the two words, and have hence taken an incorrect view of his brother's opinion, which, he adds, is a theory, not an hypothesis.

The free signification generally given to the word theory in chemical language is so well understood, that I did not suppose it could have given rise to any ambiguity requiring to be pointed out. Theory, strictly understood, implies, no doubt, a principle established by just induction from individual facts, and applied to the explanation of phenomena; while in an hypothe-

sis a principle is assumed, whence phenomena are attempted to be explained. But there is an intermediate kind of reasoning or speculation, in which there is partly generalization, partly hypothetical assumption—a principle being professedly inferred by induction, but requiring the aid of hypothesis to apply it to all the phenomena connected with the subject. To this the terms opinion, theory, and hypothesis are often indiscriminately applied; and theory in chemical language has more frequently this signification than any other. The successive revolutions in chemistry would lead us to doubt indeed if it can be often justly used in its more strict sense, implying a perfect induction, the certainty of which subsequent discoveries cannot change. The theory of Stahl, as it has been named, which at one period commanded universal assent, was quickly subverted. Much of the theory of Lavoisier, ample and conclusive as the evidence appeared to be on which it is founded, must, if late speculations be just, share the same fate. And even those more partial inductions, which appeared to have the utmost certainty, are many of them, it now appears, doubtful. To a philosophic inquirer this may perhaps suggest some caution in applying the term theory in its strict signification, and it may guard him against the most common of all errors—an undue confidence in our speculations, and the belief that the opinions of our day are demonstrated truths.

Mr. J. Davy's mistake, (and which I should have obviated in my former paper, had I supposed any one attending particularly to the subject, and accustomed to scientific deduction, could have fallen into it, and had I not wished to avoid holding out unnecessarily Mr. H. Davy's opinion as purely hypothetical,) is the supposing this opinion to be a theory in the strict sense of the term. In stating the grounds of this opinion, he exposes too, very clearly, the source of his error. Mr. Davy, he remarks, " combines oximuriatic acid gas with hidrogen gas, and forms muriatic acid gas. In his theory, muriatic acid gas is a compound of oximuriatic gas and hidrogen. He combines oximuriatic gas with sulphur, phosphorus, and the metals; and in his theory the resulting substances are compounds of the inflammable and metallic bodies respectively, and oximu-

riatic gas. Here we perceive no supposition, but a simple expression of facts, and this I humbly conceive is pure and genuine theory." It is of main importance Mr. Davy also remarks, "that expression of facts be not misrepresented. It is of great consequence that things be not termed notions, that theory be not considered as speculation."

The proper expression of the above facts is, that from the mutual action of oximuriatic gas and hidrogen, muriatic acid gas is obtained, that from the mutual action of oximuriatic gas and inflammables, or metals, such and such substances are formed. That muriatic acid is a compound of oximuriatic gas with hidrogen, or that these substances are compounds of oximuriatic gas with the respective metals or inflammables, are inferences, which may be true, or may be false. They appear no doubt to be the most direct inferences, but the most obvious and direct conclusion from an experiment may not always be the just one. The shortest way of convincing Mr. J. Davy of this will be to state to him in the very form of expression, which he employs in the above quotation, conclusions which he cannot admit. I combine, I may say, oxide of mercury and muriatic acid, and form calomel. I conclude therefore, that calomel is a compound of muriatic acid and oxide of mercury. I combine muriatic acid and potash, and by dissipation of the water I obtain a solid product, which I consider as a compound of the muriatic acid and potash: and I perceive in these conclusions no supposition, but a simple expression of facts. Mr. Davy will in this however soon correct me, and inform me not merely that they are suppositions, but conclusions altogether false. He will perceive therefore, that the inference which appears most obvious and direct is not always just; that it is sometimes necessary to take a more extensive or deep view, and will perhaps be convinced, that he has erred in the notion he has formed of the kind of induction, which constitutes a genuine theory.

The cause of errors such as this is, and which has long been recognised as the fertile source of false speculation in physics, is the taking into view only part of the facts which belong to the subject—those which appear most favourable to

our induction, instead of taking the whole into consideration, and from this general view forming the most probable conclusion. If the facts above quoted from Mr. Davy were all that are connected with the question, his conclusions might appear to be just. But there are others equally connected with it, to explain which, various hypothetical assumptions must be made, the probability of which ought to be considered, and the whole compared with any other induction that may be formed.

Thus in distilling muriatic acid from black oxide of manganese, the oxide loses a portion of its oxigen, the muriatic acid disappears, and oximuriatic acid is obtained. The direct conclusion from this experiment (and it is equally direct with Mr. Davy's conclusion from the experiment of detonating oximuriatic gas and hidrogen) is, that the oxigen of the oxide has combined with the muriatic acid, and formed the oximuriatic; and to obviate this he is obliged to *suppose*, that the oxigen of the oxide combines with the hidrogen of the acid, forming water, and setting free the oximuriatic acid. If we expose liquid oximuriatic acid to solar light, oxigen is expelled, and muriatic acid remains; and the most direct inference from this is, that oximuriatic acid is a compound of muriatic acid and oxigen. Mr. Davy must have recourse to a less obvious explanation, and *suppose* a portion of water to be decomposed, its oxigen disengaged, and its hidrogen combined with the oximuriatic acid. And he has no proof of the formation and decomposition of water in these experiments, farther than that they must take place, if his theory be true.

If we consider therefore these two opinions under different aspects, if we set out as it were from different points, each will appear an induction, which to apply to all the phenomena farther requiries some hypothetical assumptions. Considering the experiment of the production of oximuriatic acid, by distilling muriatic acid from substances which impart oxigen, the most direct conclusion is, that it is a compound of oxigen and muriatic acid; but to apply this in explaining the agencies of both acids, it is necessary to suppose in some cases changes to occur: the formation or decomposition of water for example, of which we have no independent proof. Considering

on the other hand the experiment of the mutual action of oximuriatic and hidrogen gases, the most direct inference is, that muriatic acid is a compound of these substances; but in adopting this as the basis of a theory, it is equally necessary to advance suppositions, and in particular to suppose without any actual proof, that in different cases water is formed or decomposed. These suppositions are not always required in the two systems in relation to the same fact; but still, wherever an hypothesis is required in the one, a corresponding hypothesis will be found necessary in some part of the other. They occur on the whole just as frequently in Mr. Davy's system as in the opposite one, and they are as little supported by actual proof. He, for example, has no better proof, that water is formed when muriatic acid is distilled from black oxide of manganese, than I have of its formation in the mutual action of oximuriatic gas and hidrogen; or that it is decomposed when oximuriatic acid is exposed to solar light, than I have of its decomposition when a metal is acted on by muriatic acid gas. The two opinions are perfectly alike with regard to the evidence on which they rest derived from the above facts; and to select in one of them that part where the induction appears direct, and take it for granted that it is true, the hypothetical assumptions, which must be farther made, necessarily follow: but to pursue the reverse method with regard to the other, to represent it as an hypothesis, by bringing forward the parts which require the assumption of hypothesis, and neglect or reject the more direct induction, is a mere sophism. This is exactly what Mr. J. Davy does, no doubt without being aware of it, his error arising from the confined and partial view he has taken of the subject.

All this is so obvious, that some apology is due to your readers for having illustrated it at any length. This has however been rendered necessary by the tone this gentlemen has assumed through the whole of his observations. He has held out the opinion of his brother as a genuine theory resting on indubitable evidence, and has thought himself at liberty to represent the reasoning I have employed as mere speculation, and the explanations I have here given as uniformly hypothetical, and hav-

ing on this account no pretensions to be put in contrast with the others. He will now perceive, that the subject may be presented under a different light; and he will not perhaps again hazard the assertion, that his brother's "conclusions are not tainted by the slightest admixture of hypothesis." Lest he should, permit me to add one or two illustrations, which are besides not unconnected with the subject.

On adding to nitrate of mercury, muriate of soda, nitrate of soda and calomel are formed; chemists have therefore been accustomed to conclude by rules of evidence, which they thought sufficiently certain, that the nitric acid combines with the soda, and that the muriatic acid combines with the mercurial oxide, forming the calomel. According to Mr. Davy's opinion however, though the nitric acid unites with the soda, the muriatic acid does not unite with the oxide, but these substances decompose each other, the oxigen of the oxide unites with the hidrogen of the acid and forms water, and the calomel is a compound of oximuriatic acid and metallic quicksilver. Again, on adding muriatic acid to potash we form muriate of potash, and on exposing this product to heat, so as to obtain it dry, we have hitherto believed, apparently on very strict induction, that the water was expelled, and that the dry product is a compound of muriatic acid and potash. But in Mr. Davy's new system it is supposed, that the acid and the potash decompose each other, the oxigen of the one and the hidrogen of the other combine and form water, leaving a compound of oximuriatic acid and potassium. On dissolving this in water new changes occur, the water is decomposed, the potassium receives oxigen, the oximuriatic acid, hidrogen, and a compound of muriatic acid and potash is again formed. Who does not perceive in all this abundance of hypothetical assumptions? assumptions I have no hesitation in saying more gratuitous and more complicated, than any required in the opposite system, or perhaps in any other chemical speculation.

Mr. Davy's opinion then I regard as an hypothesis; the opinion I have maintained I have also distinctly admitted in my former paper to be an hypothesis: each rests on an apparently probable induction, and is capable of being applied with more

or less probability to the various phenomena. Placing them on this ground I considered and still consider the common doctrine as superior in simplicity, in requiring less strained and less complicated assumptions, in affording explanations of facts which the other does not explain, and in according with the general system of chemical theory; receiving therefore all the support which the evidence on which that theory rests can give, while the other is anomalous, and has the weight of that evidence against it. I proceed to offer a few observations on some of the more particular topics of this discussion.

Mr. Davy remarks, that when I speak of muriatic acid I do not mean the whole ponderable part of it; water I consider as necessary to its existence in its gaseous state, and that it is to the substance free from this water that the term muriatic acid is applied. This is my meaning. But when he proceeds to the interrogations, has Mr. M. examined it in its insulated state? has he described its properties? or has any chemist ever obtained it? And when from the negative given of course to these he concludes, that I must regard muriatic acid gas as a compound of an unknown basis and water, he raises or magnifies difficulties of little importance, or at least not peculiar to this investigation. When I speak of real sulphuric or nitric acid, I mean the acid free from water, though it may not have been obtained in that state; and in thus using language which is familiar to chemists I am not aware, that I introduce any novelty of hypothesis. The real muriatic acid I consider as an acid from having reason to conclude, that it exists in its solid compounds, neutralizing the bases with which it is in combination; in the same manner as I consider real sulphuric or nitric acid as an acid from knowing, that they exist in combinations free or nearly so from water, exerting a similar neutralizing power. Mr. Davy may if he pleases suppose sulphuric or nitric acid to be a compound of an unknown basis and water, and conclude, that the supposition of the existence of these acids free from water is "speculation in the strictest sense of the word." But no chemist will find any difficulty in this. The apparently greater influence of water on the chemical powers of muriatic acid than the other acids probably arises from the facts with regard

ing on this account no pretensions to be put in contrast with the others. He will now perceive, that the subject may be presented under a different light; and he will not perhaps again hazard the assertion, that his brother's "conclusions are not tainted by the slightest admixture of hypothesis." Lest he should, permit me to add one or two illustrations, which are besides not unconnected with the subject.

On adding to nitrate of mercury, muriate of soda, nitrate of soda and calomel are formed; chemists have therefore been accustomed to conclude by rules of evidence, which they thought sufficiently certain, that the nitric acid combines with the soda, and that the muriatic acid combines with the mercurial oxide, forming the calomel. According to Mr. Davy's opinion however, though the nitric acid unites with the soda, the muriatic acid does not unite with the oxide, but these substances decompose each other, the oxigen of the oxide unites with the hidrogen of the acid and forms water, and the calomel is a compound of oximuriatic acid and metallic quicksilver. Again, on adding muriatic acid to potash we form muriate of potash, and on exposing this product to heat, so as to obtain it dry, we have hitherto believed, apparently on very strict induction, that the water was expelled, and that the dry product is a compound of muriatic acid and potash. But in Mr. Davy's new system it is supposed, that the acid and the potash decompose each other, the oxigen of the one and the hidrogen of the other combine and form water, leaving a compound of oximuriatic acid and potassium. On dissolving this in water new changes occur, the water is decomposed, the potassium receives oxigen, the oximuriatic acid, hidrogen, and a compound of muriatic acid and potash is again formed. Who does not perceive in all this abundance of hypothetical assumptions? assumptions I have no hesitation in saying more gratuitous and more complicated, than any required in the opposite system, or perhaps in any other chemical speculation.

Mr. Davy's opinion then I regard as an hypothesis; the opinion I have maintained I have also distinctly admitted in my former paper to be an hypothesis: each rests on an apparently probable induction, and is capable of being applied with more

Both are hypotheses, and if I can explain a fact by the principle of the one, which admits of no explanation by the principle of the other, the superiority of the former is with regard to this point sufficiently established. Besides, the application of the doctrine of disposing affinity alone affords an explanation of this fact. I consider it therefore as still giving a superiority to the common system, for nothing can appear more anomalous than that of all inflammable and metallic substances none but charcoal remains unchanged by oximuriatic acid, and nothing can be more satisfactory than to have a cause assigned for this. Mr. Davy considers it as of little importance, though it is this apparent anomaly, and the supposed difficulty of accounting for it according to the old opinion, which gave rise to the new doctrine, or at least first suggested the suspicion, that oximuriatic acid does not contain oxigen; he is satisfied with the observation, that we have no right to expect from a theory the explanation of ultimate facts; that is, not of comprehensive facts arrived at by a generalization which cannot be carried farther, for the fact in question is not of this kind, but of individual facts which the theory will not explain; a very safe conclusion, in which it may be proper for him to abide. This, he adds, is an ultimate fact, " one of those which constitute as it were the axioms of the science;" and he adds, that he is glad it is not tortured by hypothetical explanation. How it is elevated to an axiom, taking this word in its common sense of a fundamental proposition, self-evident, and therefore not admitting of demonstration, I do not understand; at least I do not perceive how this definition applies to the proposition, that oximuriatic acid will not combine with charcoal. Could Mr. Davy discover even a supposition by which this fact could be explained, I have little doubt that he would gladly remove it from the class of axioms, and from the facility with which he admits so many other suppositions, that he would submit it even to some degree of torture, to bend it into conformity with the system.

On the fact with regard to the influence of water in favouring the disengagement of carbonic acid from bases with which it is combined, it is scarcely necessary to make any observation.

I was aware, that it might operate by its affinity to the base, for I had suggested this some years ago, in the discussion of the question with regard to the influence of water on the constitution of elastic fluids, in my chemical system. But it farther appeared to me probable, that it operates likewise by its affinity to the acid, as there is reason I think to suspect, that the agency of water is important with regard to all acids. It is still however a case in point, since it is an example of the powerful agency of water in modifying affinities, and of a decomposition not being capable of being effected unless it be supplied; as well as a proof that a body, whether it be an earth or an acid, cannot be obtained insulated from its combinations, unless it receive the portion of water which it requires.

The argument, that from the strong affinity of muriatic acid to water a portion of it will be retained in combination with it in the elastic state, I hold valid, notwithstanding the reply, that muriatic acid does not exist as a gas in combination with different proportions of water, "for the proportion of hidrogen gas produced from muriatic acid gas, acted on by different metals, is always the same, whether it has been exposed to the influence of drying salts or not." The inference which would render this reply conclusive does not follow; admitting that muriatic acid gas cannot exist with different proportions of water, the only inference then would be, that muriatic acid gas obtained by the usual methods is in as dry a state as we can obtain it insulated. But I doubt altogether the alleged fact; for Dr. Henry found by the more unexceptionable mode of the action of electricity, that recent muriatic acid gas affords $\frac{1}{14}$ of its bulk of hidrogen, while after the full action of muriate of lime it affords only $\frac{1}{35}$ of its bulk. The inference in the reasoning on this subject, from the affinity of potassium to oxigen, arises from a misconception of the theory of Berthollet, on which the original argument is founded, so obvious that it does not require to be pointed out.

Mr. Davy proceeds to some observations on my experiments, on which I shall offer few remarks, as I shall probably have an opportunity of engaging in the discussion of this part of the subject, to greater advantage, when better acquainted with the

experiments brought forward in opposition to those I have stated. I may be allowed to say, that of the accuracy of the results I obtained, particularly of those which appear to be questioned, the formation of carbonic acid, when carbonic oxide, hidrogen, and oximuriatic acid gases are submitted to mutual action, I am fully convinced. I had preserved the notes of these experiments, some of them written by Mr. Ellis, others by myself, at the time they were made; and in all of them carbonic acid was formed, though there often remained a sensible quantity of carbonic oxide, of which I have taken notice in my paper. How Mr. Davy infers, that the whole carbonic oxide should be converted into carbonic acid, in order to admit the inference, that oxigen is communicated from the oximuriatic acid, I do not comprehend. It is sufficient, if a portion of carbonic acid is produced. No notice perhaps is due to the remark, that in one of my experiments a small residue of common air was observed, whence it is inferred, that the conversion of carbonic oxide into carbonic acid might be owing to the presence of atmospheric air. Carbonic oxide gas requires for its conversion into carbonic acid by detonation with atmospheric air a quantity equal at least to 2½ of its bulk; and not a twentieth of this quantity could have been introduced even in the most inaccurate mode, in which Mr. J. Davy may suppose the experiment to have been performed. The small portion observed had originated from the minute quantity disengaged by the force of the detonation from between the sides of the tube and the quicksilver, which, added to what might be disengaged by the transmission through the water, and agitation with it after the explosion, and to the slight admixture to which all elastic fluids are liable, (expressed in one of Mr. Davy's own experiments, by the phrase of "no more impurity than might be expected in the air in the gases"), had formed a quantity capable of being discovered. It could obviously detract nothing from the conclusiveness of the experiment, independent of the circumstance, that it was not observed in the more decisive experiment of the slow mutual action of these gases.

He is willing to admit the accuracy of my experiments, so far as they relate to the want of action between carbonic oxide

and oximuriatic acid, when water is excluded. This is a fact, which, though it presents an anomaly in the new hypothesis, is not absolutely hostile to it; and I have, it seems, " very satisfactorily proved" this nonaction, and that without the agency of water no carbonic or muriatic gas is formed. But my experiments, which afford results presenting a difficulty in the theory, are it seems inaccurate; and beside endeavouring to make this apparent by various observations, Mr. Davy informs us, that either alone, or in conjunction with his brother, he has, if not repeated them, (which would have been the most direct mode of proving their inaccuracy), at least made experiments of a similar kind, and with very different results. Thus he has performed some of the experiments of Cruickshank on the carburetted hidrogen gases. The source of fallacy, which I supposed would have been contended for, in the presence of a portion of oxigen in the composition of these gases, he seems to think of little importance; the principal source of fallacy is supposed to be in the presence of water, and he therefore made the detonations over mercury, and " never obtained carbonic acid gas, though oximuriatic gas in great excess was employed." I shall not say any thing of the source of fallacy, which I conceive has given rise to this observation, until I have repeated the experiment with the necessary precautions. I shall only observe, that Mr. Davy does not directly deny, that carbonic acid is formed, he has only performed the experiment without obtaining it, and he does not inform his readers what were the actual results. The hidrogen must of course have been removed by the action of the oximuriatic acid: What became of the carbon? Was it precipitated in the state of charcoal? Or did it remain in the state of carbonic oxide? Or did the gases form a ternary combination? and how was the absence of carbonic acid established? Yet with all these deficiencies a result thus generally stated is brought forward in opposition to experiments minutely detailed. Neither does he deny the formation of carbonic acid in my experiment of submitting carbonic oxide, hidrogen and oximuriatic gases to mutual action: he supposes its formation owing to the presence of atmospheric air, as I have already stated, or to the introduction of water after the

experiment; the gratuitous assumption being made of the formation of a triple compound of carbonic oxide, hidrogen, and oximuriatic acid, by which this water would be decomposed. Hence, in the experiment made in conjunction with his brother, ammonia was used instead of water to absorb the muriatic acid, and it is inferred, that no carbonic acid was formed. It is singular however, that no attempt appears to have been made to discover this acid, its nonformation is inferred from the gas which remained after exposure to water burning with the same coloured flame as carbonic oxide, whence it is concluded to have been this gas, with the intermixture of nitrogen from the ammonia, its volume being admitted to be different from that of the carbonic oxide employed; and this negative result, open to such obvious fallacies, is placed in opposition to the positive production of carbonic acid in my experiments. Lastly, after all these experiments made without carbonic acid being obtained, and after the labour bestowed in endeavouring to prove experiments in which it was produced inaccurate, or to account otherwise for their results, a gas is discovered (the discovery of it is mentioned in a note, it having been made after Mr. J. Davy's paper was written) which it is admitted is capable of converting carbonic oxide into carbonic acid, and which is procured from the same materials as oximuriatic acid, and by a process apparently not much different from that which is usually followed.

There remains one point to which it is necessary to give some consideration. Mr. Davy gives a reason why, as he supposes, the presence of hidrogen cannot, even on the hypothesis I maintain, favour the conversion of carbonic oxide into carbonic acid by oximuriatic acid. In submitting these three gases to mutual action, the hidrogen, in combining with oxigen from the oximuriatic, can form no more water than is required to the constitution of that portion of muriatic acid with which this oxigen had been combined, and hence there is no superfluous water to be afforded to that portion of muriatic acid, which remains to be formed in the oxigenation of the carbonic oxide by the remaining oximuriatic acid. "It is impossible therefore," says Mr. J. Davy (in the style which he permits

himself to use) "that the latter portion of muriatic acid can become gaseous;—yet it does become, according to the experiment, muriatic acid gas, which is a contradiction, and by itself a proof of the inaccuracy of the hypothesis."

This objection had occurred to me, and I had stated it as well as the solution of it to some of my friends. Of course however, a difficulty of this nature could not be urged against the fact, that the conversion of carbonic oxide into carbonic acid, which does not happen when oximuriatic gas alone is added to the former gas, takes place when there is an addition of hidrogen. And as farther the difficulty appeared to me capable of being solved, I thought it unnecessary to add to the length of my paper by taking notice of it. It is explained, I conceive, in the following manner. Muriatic acid gas may be formed with a less proportion of water than is necessary to its full saturation; the proof of its existence with an inferior proportion of water to what it contains at its first disengagement in the usual process for obtaining it I have already stated, and there is nothing improbable in the supposition, that it may exist gaseous with even less water than can be inferred from that proof. And in all elastic fluids small portions of water I believe exist, of which it is probably impossible to deprive them entirely. When a mixture, therefore, of carbonic oxide, hidrogen, and oximuriatic acid gases is submitted to mutual action, the hidrogen attracting oxigen from the oximuriatic acid, and forming with it a portion of water, this water may favour the production of a larger quantity of muriatic acid, than the quantity merely with which that oxigen had been combined, if any powerful affinity is at the same time exerted, having a tendency to produce that acid: now such an affinity is actually exerted by the carbonic oxide, hence it is enabled to operate with effect, an additional portion of oximuriatic acid is decomposed, and by the communication of oxigen, portions of muriatic and carbonic acids are formed. This is farther aided by the portion of water which must have been contained in the portions of hidrogen, carbonic oxide, and oximuriatic gases, suffering these changes, even though they had been previously rendered as dry as possible. Of itself, this would not be sufficient to favour

the decomposition of the oximuriatic acid, but added to the quantity formed by the oxigenation of the hidrogen it may be adequate to this effect, to the extent I have stated. The operation of this small quantity of water, remaining in elastic fluids in the driest state to which they can be brought, is very well shown in the results of the detonation of hidrogen and oximuriatic gases, as related by Mr. H. Davy. Mr. J. Davy, indeed tells us, that he has seen this experiment made a number of times, "and though correctly made, there is no condensation attending their union." But his brother informs us, that, when the gases are mixed over water, introduced into an exhausted vessel, and fired by the electric spark, there is a condensation of from $\frac{1}{10}$ to $\frac{1}{20}$ of the volume. I have attempted, he continues, "to make the experiment in a manner still more refined, by drying the oximuriatic acid, and the hidrogen, by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures, but I have never been able to avoid a slight condensation, though in proportion as the gases were free from oxigen or water, this condensation diminished." This condensation must be owing to the water remaining in the gases even after the action of muriate of lime; and it is this, no doubt, which partly operates in the experiments I have related, and favours the result.

Without any reference to this experiment I would farther observe, that the conversion of carbonic oxide into carbonic acid, when oximuriatic acid acts upon it with the admission of a little water, is conclusive against Mr. Davy's hypothesis. The only mode in which the result can be accounted for, in conformity to that hypothesis, is to suppose the water to be decomposed. This I had anticipated, and had observed, that of this supposition we have not only no proof, but we have sufficient reason from known facts to reject it. "Water is not decomposed by oximuriatic acid gas, or by carbonic oxide gas; there is no reason to conclude, that its decomposition can be effected by their action, when they are presented to it merely in a mixed state; and the more obvious operation may be regarded as the real one, that it acts by its affinity to muriatic acid."

In answer to this, Mr. J. Davy remarks, have we not the most indubitable evidences of the decomposition of water by oximuriatic gas? "Pass this gas and steam together through a tube heated to redness, oxigen gas will be produced, and muriatic acid gas formed. Detonate this gas and hidrogen together, and a similar formation of muriatic gas will take place. With these facts in view, who can hesitate in asserting, that water is decomposed by oximuriatic gas, that its hidrogen is attracted by this substance, and its oxigen consequently set free."

The decomposition of oximuriatic gas by detonation with hidrogen gas, or its combination with that gas, according to Mr. Davy's view, affords no proof of the power of water to decompose it. Admitting the decomposition of oximuriatic gas by water at ignition, and supposing that we know nothing as to the mutual action of these substances at low temperatures, the conclusion would not follow that at such temperatures this decomposition would take place. But to draw this conclusion in the very face of the fact itself is, I confess, a mode of reasoning altogether new to me. We know, that oximuriatic acid is not decomposed by water at natural temperatures; and knowing this I have no hesitation in asserting, notwithstanding Mr. Davy's indubitable evidences, that the supposition of water being decomposed in the slow mutual action of carbonic oxide and oximuriatic gases is altogether gratuitous, and made to avoid a difficulty, which cannot otherwise be explained.

And lastly let it be remarked, that putting aside all these experiments the two hypotheses still remain at least on equal grounds. The experiments were supposed to afford evidence in favour of the common doctrine of the nature of the relation between muriatic and oximuriatic acids; and were they proved to be inconclusive, it would only stand in the same rank with the opinion advanced by Mr. Davy; it can be applied, as I trust I have shown in my former paper, to the explanation of all the phenomena, and to some of them with more advantage than the other; and it has besides, as an hypothesis, that superiority from general considerations, which I have also stated to belong to it. I am, with much respect, your most obedient servant,

JOHN MURRAY.

A Case of Rupture of the Uterus.

From the Medical and Physical Journal for March, 1808.

Mrs. Skinner, the wife of a farmer in this neighbourhood, aged 39 years, is of a strong and healthy constitution, but was lately much weakened by a severe peripneumony. Previously to the confinement which has afforded the subject of this case, I have attended her in four labours. They were all difficult in consequence of the small dimensions of the pelvis, and in two of them, where the children were very large, and the bones of the cranium much ossified, I was obliged, on account of the dangerous state of the mother, to have recourse to the crotchet; as the most violent action of the uterus, for three and four days, was unable to bring the head so low into the pelvis as to admit the application of any other instrument, and her recovery was each time very slow and difficult. In the two other labours, the children being small, and the cranium less ossified, the delivery was effected by the action of the uterus.

She was taken in labour for the fifth time, at the full period of gestation, early on the morning of September 30 last. On my arrival, I found the os uteri fully dilated, the external parts wholly relaxed, the head of the child resting on the pubes, the membranes entire, and the pains frequent and very strong, accompanied with a small discharge of blood. During one of those pains, the membranes were protruded through the os externum, and in that state the waters were discharged. The head now gradually advanced, and, in about three hours, became forced into the narrow part of the pelvis. When it had arrived in this situation, though during every pain, it still appeared slowly to descend, she complained of a more continued and severe pain in the lower part of the uterus, with so great a degree of restlessness, that she could scarcely be prevailed on to remain in bed. As her disposition is naturally passive and accommodating, I particularly noticed this circumstance; and for the purpose of moderating the action of the uterus, I administered, without delay, a full dose of tinct. opii. The

pains, within a quarter of an hour after this, had *entirely* ceased, which I then attributed to the effect of the opiate: still, however, the restlessness was not lessened, and it was accompanied with great anxiety.

She soon afterwards called me to her bed-side, and requested me to apply my hand on the abdomen; as she expressed herself, that during the *last pain*, "something had slipped out "of its place." I perceived an irregular tumor, about the size of a man's fist on the lower part of the abdomen towards the right side. I could not distinctly feel that it contained any *particular* part of the child; but the circumstance, connected with the other symptoms, impressed me with the belief, that the uterus had ruptured at this part. The light of the morning enabled me also to observe her countenance. She was pale, breathed laboriously, and was apparently sinking. A violent vomiting supervened, the pulse was very quick and feeble, and the extremities covered with cold perspiration.

As I considered that nothing but speedy delivery could afford her the smallest chance of recovery, from a situation which was almost hopeless, I proceeded, as soon as I could obtain the proper instruments, to open the cranium, which still preserved its situation in the pelvis. But on the application of the perforator, before I could use a sufficient degree of force even to penetrate the scalp, it suddenly receded into the cavity of the uterus. I immediately followed it with my hand. The uterus was so relaxed and yielding, that I was enabled, with the greatest ease, to bring down both feet into the vagina.

It was not to be expected that the child could be extracted through a narrow pelvis in so short a time as to preserve its life, and, indeed, it was this consideration, which at first determined me on the use of the *crotchet*, in preference to turning; the application of any other instrument being, from the situation of the head, impracticable. The child proved to be so large, that it was with great difficulty I could bring the *thorax* through the pelvis, and it was not till I had applied the blunt hook over one of the shoulders, that I could bring the body so low as to reach the arms: but I was unable to deliver the head, till I had lessened its bulk, and assisted by fixing the crotchet on the in-

side. As it was in vain to wait for the natural expulsion of the placenta, I soon after the delivery, proceeded to introduce my hand again into the uterus.

If any doubt had before remained of the nature of this case, it was now wholly removed, as I passed my hand without difficulty, through the aperture into the abdomen. I applied my fingers on the smooth *internal* furface of that cavity, when the gentleman who assisted, and myself, were both satisfied, by placing our hands, at the same time, on the *outside*, that nothing but the thin parietes of the abdomen intervened. The ruptured part, as nearly as I could ascertain, was in the anterior part of the cervix uteri, extending also into the vagina. There was fortunately none of the intestines in the way, nor did I think myself justified in searching for them, but hastened to withdraw my hand into the uterus, when I removed the placenta, and resigned, as I had then too much reason to fear, my unhappy patient to her fate.

No extraordinary discharge followed the delivery. She continued during the remaining part of this day in a very languid state, with violent and frequent vomiting.—On the following morning, having had some sleep during the night, she appeared considerably revived. The pulse was 120 in a minute, and stronger. There was no general enlargement, nor pain in the abdomen, except on pressure, when the uterus, was described to be exquisitely tender, and was felt to be contracted about the size of a child's head. She had voided urine without difficulty, but there had been very little discharge of lochia, and scarcely any thing had remained on the stomach. In this state, she continued with little variation for the first four days. —The regimen during this time was strictly antiphlogistic, and the medicines were saline draughts, with gentle aperients, and an opiate at night.

From the 5th to the 12th day, she became gradually into a state of the most distressing restlessness and anxiety: constantly endeavouring, but unable, to change her posture. She repeatedly vomited a *dark coloured* fluid, and had frequent motions of a similar appearance. The pulse was very irritable and weak, and varying from 130 to 150 in a minute; the abdo-

men was enlarged and painful; the uterus extremely tender; the lochia serous, and in very small quantity.—The Peruvian bark in different preparations was now directed to be taken freely, and light nutriment, in all the forms that could be devised, was prescribed for her. But almost every thing was ejected from the stomach, and except the opiate at night, she could not, at last, be prevailed on to take any medicine, and with great difficulty, food. A few hours' sleep, procured by the opiate, was the only relief she experienced.

Such was her situation on the 12th day, at which time the size and tension of the abdomen indicated an extravasation of fluid into that cavity. As no medicine could be retained on the stomach, the ungt. hydrargyri fort. was directed to be rubbed into the abdomen twice a day, with a view, if possible, to produce its absorption.

On the following morning, the enlargement was much subsided, in consequence of her having had several *very profuse* motions during the night. The uterus was become less tender; the pulse reduced to 100, and the restlessness, anxiety, and every other unfavourable symptom, abated. From this day, she continued gradually to amend, but whether this abatement of the unfavourable symptoms was occasioned by the mercurial action, or whether the application had any effect in producing the extraordinary discharge from the bowels, is uncertain. There appears, however, to have been scarcely time, nor does it seem that the quantity used was sufficient for so powerful an operation.—Yet though I was more disposed to attribute it to a critical effort of nature, than to an effect of the medicine employed, I considered it, at least, as an inducement to continue its use: and the amendment taking place so soon after its first application, my patient was very solicitous to persevere in it. In eight or ten days, a soreness of the gums obliged me to desist. During this period, a larger discharge of lochia came on; the stomach by degrees retained food, and the more alarming symptoms had so far subsided, that I could not but indulge the most pleasing expectation of her recovery.

About three weeks after delivery, she was attacked by the peculiar swelling of the leg and thigh incident to lying-in women, (Phlegmatia Dolens.) This considerably retarded the

progress of her recovery, and did not wholly subside for another fortnight.

The debility occasioned by so long and painful a confinement, may be supposed to have been very great, and it was not till five weeks after delivery there could be said to be any return of strength. She was enabled then to be taken out of bed for an hour daily, and her appetite began to increase. The swelling of the abdomen was entirely reduced, but there was still felt, on pressure, a tenderness in the region of the uterus. She also complained of a very troublesome sensation near the spine of the left ilium, when in an erect posture, or when lying on the opposite side, which she described "as if something was drawing away from that part:" probably, the consequence of an adhesion formed there, during the period of peritoneal inflammation. She went on rapidly, from this time, in a state of convalescence, and is now so well recovered, as, except a diminution of strength, to feel little inconvenience from the accident.

The above is an addition to the few instances of recovery, with which we are acquainted, from the most formidable accident that can occur in the practice of midwifery; and though there may be but little practical information to be derived from it, the recording of it must tend to inspire us with a greater degree of confidence in the powers of nature, even in the most desperate cases, where the impediments to the exertion of those powers are removed.—The only two instances of ruptured uterus which have fallen under my own observation have been in women with narrow pelves, and in both of them it took place in or near the cervix uteri. A degree of weakness in any part of that organ, disproportioned to the resistance to be overcome, must always predispose it to rupture. But if the weakness of structure should exist principally near its *cervix*, that part, *after the evacuation of the waters*, is in danger of becoming violently compressed between the head of the child and the edge or some projecting part of the pelvis; and in such a situation, the probability of its giving way must be very great. Whenever, therefore, delivery is protracted on account of an extraordinary degree of resistance, either from the smallness of the pelvis,

or a rigidity of the soft parts, the practitioner has been much cautioned to avoid the premature rupturing of the membranes. And if, *after that has taken place*, the woman should complain of an unusually severe pain, confined to a particular *part* of the uterus, distinct from, more agonizing, and sometimes more constant than the labour pains, symptoms which indicate the too forcible compression of some *portion* of the uterus, he cannot be too much upon his guard, nor too prompt in endeavouring to moderate its action, and to lessen the resistance it has to overcome.

Both these purposes, if the difficulty should depend solely on a *rigidity of the parts to be dilated*, may, as far as it is in our power, be accomplished by large doses of opium, with blood-letting in proportion to the strength of the patient. But, as is more frequently the case, if the resistance should be occasioned by the *narrowness of the pelvis*, it becomes a subject of consideration, whether the magnitude of the danger we apprehend and wish to avert, will not justify us in effecting the delivery without further delay: by turning, if it be thought compatible with the safety of the mother and the child, or otherwise even by lessening the bulk of the head, should it be situated too high to admit of our applying the forceps or the vectis.

When, notwithstanding all our precautions, a rupture of the uterus has unhappily taken place, the necessity of a speedy delivery is, I believe, generally considered as unquestionable.— Every additional case of recovery, under these circumstances, should impress us the more strongly with the propriety of the practice, and deter us, under an idea of the unavoidable fatality of the accident, as has been sometimes the case, from allowing the woman to remain undelivered, which can afford the least possible chance to the mother, and must *necessarily* prove the destruction of the child. I am, &c.

THOMAS HUGO.

December 24, 1807.

A Case of Intus-Susceptio,

WITH REMARKS, BY THOMAS BLIZARD, ESQ. F. R. S. AND R. S. EDINBURGH, AND SURGEON TO THE LONDON HOSPITAL.

From the Medico-Chirurgical Transactions, Vol. I.

HAVING lately had an opportunity of examining the body of a male child, aged five months, who died in consequence of an intus-susceptio, I take the liberty of transmitting an account of the appearances, with a drawing by Mr. Clift, to the Medical and Chirurgical Society.

Intus-susceptions, to a small extent, are not unfrequently noticed, particularly in children, without any mark of having produced obstruction or inflammation of the alimentary canal; but, as a disease, it, happily, is of rare occurrence. The subject of the present case had, previously to the attack, been always healthy, and free from bowel-complaints. On Sunday, the 28th of February, he was seized with vomiting, accompanied with constipation, and other signs of disordered functions of the bowels. There were, at first, some small discharges of mucus by the anus; but, after Monday, the discharges, which were frequent, were principally of blood. The abdomen was tense, and on the left side, there appeared a tumor, about the size of an egg. A hiccoughing commenced on the Tuesday, and continued until death, which took place on Thursday evening, the fifth day from the accession of complaint.

On dissection it appeared that the tumor on the left side was produced by an intus-susception: about six inches of the intestinum ileum, the cœcum, with its appendix, the ascending colon, and transverse flexure, being contained in the sigmoid flexure of the colon, extending into the rectum. The intus-suscepted parts were in a state of complete strangulation, and perfectly black. The lower part of the ileum (about ten or twelve inches) immediately above the intus-susception was a little inflamed; but, otherwise, the effects of this derangement of parts were so strictly confined to the intus-suscepted bowel, that had the child's constitution been able to sustain its separation, the inflammation necessarily accompanying this process would, no doubt, have produced an union of the ileum with the lower

part of the colon; the continuity of the canal would thus have been maintained, the separated part might have passed, and the child have recovered.

I feel justified in hazarding the opinion of the possibility of the child's doing well under these circumstances from the relation by Dr. Baillie, in the second volume of the transactions of a society for the improvement of medical and chirurgical knowledge, of two cases in which gut was passed per anum.

The one case is that of a lady, about fifty years of age, who, after much suffering with violent pain of the stomach and bowels, more especially on the left side, accompanied with vomiting and constipation, about three weeks before her death, voided above a yard in length of intestine, which proved to be a portion of the colon. It is worthy of remark, that the pain was more especially seated on the left side, and that the evacuations, for many days, consisted merely of blood, and at that time were very numerous. In these respects there is a striking resemblance between this case and the child's just related.

Of the other case there are no further particulars stated, than that the person lived two years after discharging a portion of gut, about six inches long, and which proved also to be a part of the colon.

The learned doctor not having had an opportunity of investigating after death the processes of nature under these extraordinary circumstances, has proposed an ingenious explanation of the cases by supposing a membrane, formed of coagulable lymph, to have been produced round the mortified portions of intestine, by which a continuity of the canal was maintained. It is with the greatest deference I presume to offer a sentiment contrary to an authority so generally, and justly, respected; but is it not highly probable that these were cases of intus-susceptions, in which the intus-suscepted portions had been strangulated, had died, and were separated?*

* I am authorized by Dr. Baillie to say, that for a considerable time past, he has entertained the same ideas on the subject, which I have here stated. I was not aware, when I wrote this paper, of Dr. Hull's having some years ago, (in the Medical Journal for 1802,) adopted a similar view of the point in question; and of Dr. Baillie's having then, with the most honourable candor, addmitted that he was probably mistaken in the reasoning which he had em-

It is not very easily to be conceived, how, in the natural state of parts, the peritoneum, which binds down the cœcum and ascending colon, admitted of their being suddenly removed, as in the present case, to the left side into the sigmoid flexure of the colon: it is therefore very probable, that there was some variety in the mode of application of the peritoneum here, and which, if similar to what I once, some years ago, observed in a subject on which I was demonstrating, would readily permit such a displacement and might strongly predispose to it. The peritoneum was applied in a loose form to the cœcum and ascending colon, being, as it were, a continuation of the mesentery: by which these parts were, in some degree, left loose, and might easily pass over to the left side.

ployed with regard to it. I am happy in noticing this coincidence of opinion between Dr. Hull and myself, and in furnishing a case which tends so much to confirm its accuracy.

SELECTED REVIEWS.

Practical Observations on the Formation of an Artificial Pupil, in several deranged States of the Eye; to which are annexed, Remarks on the Extraction of Soft Cataracts, and those of the Membraneous Kind, through a Puncture in the Cornea, illustrated by Plates. By BENJAMIN GIBSON, Vice-President of the Literary and Philosophical Society of Manchester, and Surgeon to the Manchester Infirmary. London, 1811. pp. 154.

From the New Medical and Physical Journal for June 1811.

THAT, in cases of imperfect vision, the sight of a patient might be improved, and those of total interruption of sight might be restored, by forming an *artificial pupil*, has long been known; for Cheselden performed the operation with success, and his operation has been repeated by several surgeons since his time. The mode of forming the artificial opening has also been varied, and sometimes success has attended the operation; and at other times the attempt has failed, for want of a due discrimination of the causes producing the defect, and of the actual state of the parts concerned in the deranged state of the eye. Our author has entered into a minute examination of these circumstances, and has so clearly explained the actual states of the various parts of the eye in the different cases of disease, so ably described the mode of relief adapted to each, and has so happily illustrated his opinions by his successful practice, that we do not hesitate recommending a perusal of his work to all who are concerned in the treatment of diseases of the eye, which unhappily have become sufficiently prevalent in this country, from the time the British troops so gallantly reaped unfading laurels on the contested plains of Egypt.

There are several states to which the eye may be reduced by disease or accident, which can only be relieved by the formation of an artificial pupil. An opake state of the cornea, however, is by far the most frequent; and this may be complicated with disorganization and derangement of the internal parts of

the eye. It is chiefly in those cases, where the opacity occupies the centre of the cornea, leaving it more or less transparent towards its circumference *only*, that light is prevented from passing through the pupil, and vision so far impaired, as to require the assistance of art. When sight is lost from such a cause, it may be restored in a great measure, by the removal of a portion of the iris, opposite to the transparent part of the cornea, so as to form what has been termed an artificial pupil.

The varieties of derangement requiring an artificial opening, and in which he has operated, are stated by the author to be the following. Opacity in the centre of the cornea; central opacity of the cornea, attended with adhesions between the cornea and iris, which include only a portion of the inner border of the iris; central opacity of the cornea, attended with extensive adhesions to the iris, which include its whole border, or nearly so; central opacity of the cornea, and total opacity of the crystalline lens, or its capsule, with or without adhesions of the iris to the cornea; and, total closure, or obliteration of the pupil, attended with entire transparency of the cornea.

"The mode of operation for forming an artificial pupil, it is obvious, must be varied according to the circumstances of the case. When there exists merely an opacity in the centre of the cornea, without derangement in the internal parts of the eye; or when the pupil is closed after the extraction or depression of a cataract; in these cases the operation will be the most simple. But if, along with central opacity of the cornea, the iris have formed adhesions to its inner surface, with or without a corresponding diminution of the capacity of the anterior chamber of the aqueous humor, the operation becomes more complex. If again, along with opacity and adhesion of the iris, the crystalline lens or its capsule be opake, the adoption of still more complex expedients is necessary. The same may be observed of those cases, in which the pupil is totally obliterated or nearly so, and the iris adheres to the capsule of the crystalline lens; for this rarely happens without an opacity of the lens or its capsule, or of both. Sometimes however, a blow upon the sclerotic coat, particularly affecting the crystalline lens, and probably rupturing the edge of its capsule, is followed by the absorption of the lens, and leaves only the opake capsule."

The instruments employed by Mr. Gibson, are, in addition to the cornea-knife and curved scissars, recommended by Mr. Ware,

"1st. The very small hook, which is generally found in a case of extracting instruments.

"2dly. A small pair of forceps, which shut by a spring, and require the pressure of the finger to open them. Thus constructed, they are not affected by the pressure of the lips of a small incision in the cornea, through which they may be introduced; and when once a fragment is laid hold of, within the eye, the pressure of the spring will retain it between the blades, without the assistance of the fingers. One of the handles of the forceps is of considerable length, and the other short. By this means the forceps can be held steadily between the fingers and thumb, whilst the fore-finger is applied to the shorter handle, to open them occasionally.

"3dly. A small pair of iris-scissars, with one blade blunted at the point. They have handles like the forceps, but differ from them in this respect, that they open by means of a spring. These scissars are sufficiently minute to enter a small incision in the cornea, and to clip off portions of the iris, or of membraneous cataracts."

According to our author's experience, the permanency of the artificial pupil depends principally upon the size of the opening and healthy state of the iris and contiguous parts of the eye at the time of the operation. When the artificial pupil has been made almost as arge as the medium size of the natural one, and especially when the part of the iris removed has included its border, there appears no disposition in the opening to close. When, however, a mere narrow slip has been removed, when the iris, from previous inflammation, has become more vascular, than natural, or when it is complicated with adhesions to the capsule of the crystalline lens, in such cases its closure has occasionally taken place.

It does not appear that after the forming an artificial pupil the fibres of the iris possess any power of motion, so as to alter the size of the new opening. Mr. Gibson, when circumstances allow it, always prefers forming the artificial pupil to-

wards the external angle of the eye, and thinks that when the pupil is formed towards the internal angle, the sight is less extensive, the nose in some measure appearing to curtail the sphere of vision.

The following history will show Mr. Gibson's mode of operating, and the success attending it; it is not a partial selection he has here given us; for similar success, he assures us, has been almost uniformly the result of every operation of this kind, where the previous state of the eye afforded much expectation of deriving advantage from the operation.

"William Crooke applied at the Manchester Infirmary in May 1804, on account of blindness, which had existed for several years. On inspection, the pupil of each eye was found entirely closed, so that the state of the crystalline lens, or its capsule, could not be discerned in either. As, however, his loss of vision was occasioned by violent internal inflammation of the eyes, it was concluded that adhesions had formed between the opake capsule of the lens and the posterior surface of the iris, and that the crystalline lens was most probably in an opake state; as had uniformly been observed in a variety of similar cases.

"The operation, in this case, consisted of two stages. In the first, the substance of the crystalline lens was broken down in each eye. In both instances, it was soft, and readily yielded to the pressure of Mr. Hey's couching needle. The right eye was irritated, and some inflammation followed, which required the use of leeches, purgatives, and cooling collyria.

"The second part of the operation was performed in about a fortnight, when the eyes had perfectly recovered from the inflammation produced by the couching needle. It consisted in entering the knife through the cornea, into the anterior chamber of the aqueous humor, at the usual part. The point of the instrument was next passed through the iris, at the distance of about one third of its diameter from its root, so as to form an opening, whose length was also about one third the diameter of the iris, as nearly as could be calculated. The cornea-knife was then gently withdrawn, and the iris-scissars were introduced shut, until they reached the new formed opening in the

iris. They were then opened, and one blade was passed through the aperture formed in the iris, and the other blade directed anterior to that membrane, until the middle third of the iris was included between them. An incision was now carried through the included portion, commencing at the upper part of the opening made by the cornea-knife, and directed a little obliquely downwards; so that when a similar incision was made from the inferior part of the same opening, the pupil formed an irregular aperture, resembling in some degree an equilateral triangle.

"As soon as this portion of the iris was cut out, the opake fragments of the crystalline lens presented themselves, in a pulpy state, and were removed through the new pupil by means of the curette, until an inconsiderable portion only remained. The aqueous humor being in great plenty, the remaining fragments of the lens disappeared in the course of a week, and no further operation was necessary upon this eye.

"The right eye, which was not operated upon with the iris-scissars, until the left eye had recovered, required the scissars to be used twice, with some interval; principally, because the new formed pupil had contracted into too small bounds, and was not, therefore, similar to the pupil of the left eye. The opake fragments of the lens, also, had not a sufficiently free egress from behind, into the anterior chamber of the aqueous humor. By a second operation with the iris-scissars, the pupil was enlarged, and the whole of the opake fragments were removed by the curette. Some degree of inflammation again attacked this eye; which, however, is a rare occurrence after this operation. This patient returned home, and was able to follow his occupation as a weaver."

The author then proceeds to detail the circumstances under which little benefit is to be expected from this operation, and wherein he would not interfere, but at the earnest request of the patient. No uncommon effect of the Egyptian ophthalmia is to leave an opacity of the cornea through its whole extent, except an inconsiderable portion at its circumference, not exceeding in length or breadth one quarter the diameter of the cornea, which remains tolerably transparent. The opake por-

tion is generally thicker than common, and irregular upon its surface; in the majority of such cases, the means adopted induce a degree of inflammation, which the diseased structure of the cornea cannot withstand, and consequently such a state of the parts is unfavourable to the operation. However anxious the patient may have been for the operation, or however plainly its danger may have been pointed out to him, he never fails, Mr. G. remarks, to regret the loss of a scanty perception of light, which he enjoyed before it was undertaken.

The remainder of the volume is occupied by observations on the mode of extracting the soft and membraneous cataracts through a puncture in the cornea. These are highly interesting by their novelty and importance, and we must refer to the work itself, such as are desirous to peruse them. The following case of membraneous cataract illustrates Mr. Gibson's method of procedure, and may be taken as a fair estimate of his usual success in this operation.

"Millicent Smith applied at the Manchester Infirmary, on account of a considerable defect in the sight of one eye, the other having been destroyed some years before. On examination, an opake speck was observed in the centre of the capsule of the crystalline lens, surrounded by a slighter degree of opacity, of small extent. This state of the capsule occasioned a greater defect in vision, than might have been expected, especially as the eye appeared quite healthy in other respects. Although the crystalline lens, in this case, was most probably free from opacity, yet its removal was necessary; because the speck and opake portion of its capsule could not have been removed, without producing opacity in it.

"The operation, therefore, consisted in first attempting to depress the lens; this, however, proving too soft, was broken down with the couching needle, and its capsule freely ruptured to admit the aqueous humor. When the eye had recovered, a puncture was made in the cornea, through which the lens was removed in a soft state by the curette, and the speck and opake capsule were extracted, partly by the hook, partly by the forceps. The pupil became quite clear, and the girl left the hospital with as good sight as patients usually enjoy, after the removal of the crystalline lens."

Two plates accompany these observations; one represents the instruments employed by Mr. Gibson in the operations described, the other shows the appearance of the artificial pupil in the inner margin and centre of the iris. The volume, we are confident, will form a valuable addition to the library of the surgeon.

Observations on the Natural History, Climate, and Diseases of Madeira, during a period of eighteen years. By William Gourlay, M. D. Fellow of the Royal College of Physicians, Edinburgh; and Physician to the Factory at Madeira. 8vo. pp. 158. Callow, 1811.

From the Medical and Physical Journal for May 1811.

FROM the opportunities for observation and practice which the author's long residence in Madeira has procured him, we anticipated copious and valuable information respecting an island of which we hear much and know little. When chagrined with want of success, and wearied with the progressive increase of all the patient's symptoms, we propose a voyage to Madeira as a last hope, although we are no f urtheracquainted with the country, than having some confused notion that the climate is mild.

The first part of this volume treats of the discovery, situation, aspect, and soil of the island; of its vegetable and animal productions; and of the constitution, customs, and manners of the inhabitants.

"Madeira is situated in 32 degrees, 37 minutes, 30 seconds, north latitude, and in 17 deg. 5 min. longitude, west of Greenwich, about 80 leagues N. by E. from Teneriffe, 120 leagues from Cape Cantin, on the coast of Africa, nearly 100 leagues from the Isle of Terno, and about 17 leagues S. W. from Porto Santo. It is about 120 miles in circumference, its greatest length from E. to W. being 45 miles, its greatest breadth from S. to N. 15 miles, and its least breadth 8½ miles. It is formed of lofty mountains, of hills, and fruitful valleys, and in figure makes an oblong irregular quadrangle. Its capital is Funchal, which is

situated on the south side of the island at the bottom of a spacious valley, open to the sea, and surrounded by lofty mountains, having all the appearance of an amphitheatre, gradually ascending to a great height. Its mountains and hills generally rise with a slow ascent; the highest points of land being about 8250 feet, or one mile and a half, above the level of the sea. The situation of Madeira, in some places, presents a most picturesque and enchanting appearance, while in others, huge perpendicular rocks, lofty precipices, prominent ridges, deep excavations and chasms, innumerable cascades liberally supplied with rivulets, beautiful valleys, deep gulleys and ravines, containing immense torrents of water, afford a highly varied, sublime, and no less alarming picture of nature." P. 5—6.

This island was formerly extremely fertile, and though the soil has been exhausted by a succession of crops for nearly four hundred years, without much assistance from culture it still nourishes a variety of vegetable productions; all sorts of tropical, as well as European fruits, will flourish on this favoured spot, and every kind of grape grows there.

Cattle, poultry, fish, &c. &c. are very plentiful; and various species of small birds are seen. We wish that Dr. Gourlay had enabled us to detail more particulars of the natural history of Madeira; his information on this head is very scanty, although many rich and curious productions of nature are to be met with in the island.

His account of the inhabitants is interesting.

"The natives of Madeira, particularly the peasantry (he observes) are distinguished by an olive or tawny colour of skin, and a swarthy complexion; nor is it improbable that they are of a mulatto or Moorish origin. Indeed only a few of the first families at all resemble in complexion the fair inhabitants of northern Europe, and these are unequivocally of Portuguese extraction. The people of Madeira are, in general, athletic, well made, active, and of a middle stature. Those of the lower class, or the country people, are sober, inoffensive, economical, and capable of enduring much hard labour; in the prosecution of which they are often reduced to great emaciation of body, and debility of constitution, and thus a premature old age is

brought on. The higher classes, on the contrary, are inclined to corpulence, and at the same time more disposed to indolence, attended with a moroseness of temper, and disposition to melancholy: though sober in respect to drinking, they are apt too often to indulge in eating to excess; from this circumstance, joined to the sedentary life they lead, they become subject to a variety of chronic disorders, and also early arrive at a premature old age; nor is it to be concealed that of late years the use of spirituous liquors has become common here among all ranks, which has opened a new field for the production of a long train of maladies." P. 27—28.

The women marry early, are very prolific, and soon lose the charms of youth. Their habits are adverse to health, leading very sedentary lives, seldom going out but to church, and performing many religious exercises. The younger branches of the first families are immured within convents, from which, after taking the veil, they never issue.

The climate of Madeira forms the subject of the second part of this work, and extracts from a Meteorological Register, kept during a period of sixteen years, are inserted. The temperature appears to be pretty uniform, the hottest months being rendered pleasant by a succession of land and sea breezes. The following table from Kirwan's "Temperature of different latitudes," presents the average temperature of every month in the year.

"Madeira, Funchal, lat. 32 degrees 37 minutes, long. 17 degrees, mean height of the thermometer for every month, taken from an average of four years' observations.

Jan.	64,18.	July,	73,45.
Feb.	64, 3.	August,	75,02.
March,	65, 8.	Sept.	75,76.
April,	65, 5.	October,	72, 5.
May,	66,53.	Nov.	69,08.
June,	69,74.	Dec.	65.

The following is the average temperature of Madeira, compared with that of London, for the whole year, as well as during the coldest and warmest months, which are January and

July. Taking the average temperature of London at 1000, the heat of Madeira is 1319. In January 0559, July 1128."

The third part treats of the diseases of Madeira; these the author arranges under the heads endemic and epidemic.

The first division comprehends various affections of the skin, complaints of the chest, rheumatism, colic, tetanus, dropsy, &c. Dr. Gourlay begins with describing elephantiasis, or Arabian leprosy, which is very common among the lower classes of people in Madeira. The disease is hereditary, and may be communicated from one person to another, although it does not appear to be of a very contagious nature.

"It generally shows itself by tubercles upon the face and upper extremities, and sometimes upon the trunk of the body and penis. Illconditioned ulcers of the legs also take place, in some instances, attended with acute pain; large indolent glandular tumors occupy the upper and anterior part of the thigh. The fingers become contracted, and the feet hard and swelled. The fingers also, and toes, are occasionally destroyed by ulceration; the same disposition to irregular tumors and ulceration attacks the throat.

"In those affected by the disease, previous to the age of puberty, the usual signs that mark this period of life do not appear. The beard, the usual sign of virility, is wanting; the hair is deficient on the pubis and scrotum, as well as on the axilla and breast. No desire prevails for the venereal passion: the voice preserves its puerile tone, and does not acquire the real strength and masculine expression. Even the testicles, not called upon for the exercise of their functions, gradually waste." P. 68, 9.

Young females affected with this disease experience no increase or fulness of the breasts, no growth of the external parts of generation, no appearance of the menses, have no hair on the pubis or axilla, and feel no disposition to venery.

"Even where the disease makes its first attack, at a much later period, the marks of sexual maturity, which are already established, gradually disappear, and are attended with impotence or very impaired powers of generation.

These circumstances denote that the elephantiasis of Madeira differs from the same complaint in other countries: for almost every writer on the subject has described the unhappy victims of this disease, raging with insatiable irresistible desire for venery; *rabiosa inest libido.* In support of the contagious nature of this disease, Dr. Gourlay states, that,

"About thirty years ago, in the village Ponta de Sol, fourteen miles distant from Funchal, the capital of the island, (Madeira) it raged with all the violence of an infectious malady, passing from one family to another, and threatening to extend its ravages into the neighbouring country, till the governor took this (the) prudent and wise step of separating the healthy from the diseased, and preventing their interference with each other." P. 72.

Some cases also under the author's immediate notice are cited in favour of this opinion. Where it affects people who have previously been healthy, the mode of attack resembles that which distinguishes contagious diseases. "It is ushered in with rigors and other symptoms of pyrexia, while at the same time there is no evidence of the presence of any other existing morbid cause." We have hitherto regarded this disorder as an incurable one; Dr. Gourlay however has occasionally succeeded in affording relief. In the incipient state, he has in a few instances suspended the progress of the disease by the use of calomel. In other cases he found great benefit follow the internal administration of the lacerta (agilis) or common lizard.

"As a medicine, this reptile acts as a powerful stimulant on the living solid, opening the several excretions, and producing large evacuations, particularly by the skin and urine, which are at the same time not attended with any debilitating effect. By this mode of operation, it will be found to have the certain influence of arresting the progress of the worst symptoms of elephantiasis, if not the whole, and in many cases to have surprisingly restored parts, which for years had been morbidly enlarged, to their natural size, and even sensibility; though for that period they had continued in a torpid state. Its operation also seems to vary somewhat in different cases; at times

the different secretions seem increased by it all at once, viz. the perspiration, urine, and saliva; at other times, merely an increase of saliva takes place." P. 74.

Diarrhœa sometimes followed the use of this remedy, and it occasionally produced vertigo. Some successful cases are subjoined from which we are convinced that this reptile possesses considerable efficacy in the cure of elephantiasis: It has also been administered with advantage, in herpes, chronic rheumatism, dropsy, and scrofula.

As most of the diseases in Madeira are similar to those of this country, we shall confine our extracts to such as present unusual appearances, or have some peculiar claim on our attention. Dr. Gourlay mentions an inveterate species of psora, termed by the natives, ouçao. It is well described by Dr. Adams in his work on morbid poisons. This affection usually attacks children at an early age, and is evidently occasioned by small animalcules piercing the skin. They possess the power of leaping like fleas; are somewhat larger than cheese mites, and belong to the genus acarus (siro exulcerans). Linn.

"The first appearance of the disease is in the form of a pellucid watery vesicle, attended with intolerable itching, and which, on being rubbed, breaks and discharges a thin watery fluid. A crust or scab is afterwards formed, from under which there is again emitted an acid ichorous matter. This matter corrodes the neighbouring parts, and tends to extend the disease, which is further assisted by the ova of the original infectious animalcules, and the locomotive power they are ascertained to possess."

The cure is most frequently performed by means of extraction with a pin or needle; but the complaint is easily removed by the application of mercurial and sulphur ointments.

"From a similar cause of animalcular irritation is derived another cutaneous malady, known by the name of alfora, from the small winged insect which occasions it. This insect is about the size of the one described, as producing the itch, and the affection it intails is most troublesome during harvest, and immediately after it." P. 85.

From cutaneous affections, our author proceeds to diseases of the chest, which in spite of its boasted salubrity are common in Madeira; the natives even are often affected with pulmonary consumption.

"Persons of all ages, and of both sexes, fall victims to it; nay, whole families have at times been suddenly swept away by it.

"The species of the disease that produces the ravages here, is that connected with scrofula, a disorder equally common here as in the colder regions of Europe: it uniformly at first assumes the appearance of a mild catarrh, but afterwards, when the real pulmonary symptoms commence, they prove more violent and rapid in their progress, than in the phthisis of northern climates."

An idea prevails in Madeira that this disease is of a contagious nature, and on that account the inhabitants of Funchal will hardly under any circumstances receive a phthisical patient into their houses.

The proper period for consumptive persons to leave England is the month of October, the fittest season in Madeira being from November to the beginning of June. In the treatment of phthisis, Dr. Gourlay experienced the most success from a cautious administration of digitalis. In the incipient stages, he generally effected a perfect cure; in the more advanced stages great relief, and in some instances even complete restoration to health, by the use of this remedy.

In the second division, epidemics, the author classes those diseases which arise from a specific contagion. Fever is frequent in the island, and it usually assumes the typhoid character. Dr. G. attributes the fatality of this complaint to bad practice, and especially to bleeding. This is often the case in the country; but in towns, under regular practitioners, the termination is generally favourable.

In 1806, scarlatina was epidemic the first time in Madeira, and attended with a great fatality.

"The characteristic symptoms which marked its attack, were inflammation of the tonsils, and mucous membrane of the fauces, attended with extensive and repeated sloughing of

these parts; eruption of the skin, varied in its appearance, form and extent, in different cases, and great debility of the whole of the functions. The affection of the throat, however, was by no means a constant symptom, and the attack was as frequently without it as with it.

"At its commencement, so contagious was the nature of this epidemic to appearance, as to be considered as the epidemic or contagious catarrh combined with quinsey, and in other cases, as measles; and indeed from the very variable mode of its attack, though its nature soon ceased to be in the least doubtful to an experienced practitioner, still it could not fail, from its incipient appearance, to deceive one who looked only to the regular and usual form of scarlatina. In many cases, for three or four days, delirium was the only symptom of the disease, attended with anxiety of the precordia, dyspnœa, palpitation of the heart, cough, bilious vomitings, edematous swellings of different parts of the body, and, in proportion to the violence of these symptoms, suspension also of sense and motion. In other instances, the malady was ushered in by violent hemorrhage from the nose and mouth, attended with a quick feeble pulse, and occasionally frequent fits of syncope." P. 111,112.

It chiefly affected children: we would willingly transcribe this author's very minute account of the symptoms, in the enumeration of which, he has evinced great accuracy of observation; but we have already rendered this article long. The treatment pursued seems to have been judicious, and we were gratified to find that in the cases where cold affusion was freely administered, considerable relief was afforded.

In the autumn, dysentery generally prevails, and is highly contagious. Smallpox was formerly very destructive in this island, but the practice of inoculation has now checked the mortality; and vaccination promises to be successful in further diminishing it. Dr. Gourlay states, that he has not failed in a single instance in producing the disease, and that none of his patients, though afterwards subjected to the contagion of smallpox, have been affected by it.

Measles are frequent among the children in Madeira; the inflammatory stage is very short, and soon succeeded by a de-

gree of typhoid debility, from which the chief danger arises. The submaxillary glands often swell and prove troublesome. Hooping-cough is epidemic, and the paroxysms are so violent as frequently to terminate in apoplexy. Emetics administered in the commencement of the complaint pro ce the most benefit.

An appendix contains a short account of the mineral waters in the Portuguese Island of St. Miguel.

Upon the whole we have been gratified by the perusal of this little volume; should a second edition be called for, we hope the author will endeavour to make the department allotted to the natural history of the island more complete. We cannot avoid also remarking, that besides a full page of errata enumerated, various other errors, both of the press and of the editor, have escaped correction.

ORIGINAL PAPERS.

Some account of the use of the Polygala Senega in obstinate Amenorrhœa.

BY N. CHAPMAN, M. D.

It has been remarked by the celebrated Dean Swift, with his usual propensity to epigrammatic point, that he who raises two sprigs of grass where only one had previously grown, confers on his country a more substantial benefit than is derived from all the achievements of arms.

To the same species of praise, and certainly not to a less share, is the physician intitled, who, by new discoveries, adds to the catalogue of means by which the maladies of our nature are mitigated or cured.

Every practitioner of medicine, who is much consulted in the diseases of women, must have experienced the difficulty of managing some of the forms of amenorrhœa, by the ordinary remedies. In a conversation with my friend Dr. Hartshorne, about two years ago, upon this subject, he mentioned to me that he thought he had used, with advantage, a decoction of the Senega in these cases. Confiding in the accuracy of his observations, I determined to lose no time in making a trial of the medicine. This I have since done, both in my private practice and in that of the Almshouse,* to a considerable extent, and with sufficient success, to warrant me in recommending it as among the most active and valuable of the emmenagogues. The ensuing cases, selected out of a much larger number, will serve not only to attest the decisive efficacy of the medicine, but may, perhaps, also, in some measure, illustrate the particular state and circumstances of this deranged function, to which it is best adapted.

The first case in which I gave the decoction, was that of a woman, aged thirty years, by trade a sempstress, of a

* The Almshouse, as the name imports, is an asylum for paupers. There is, however, a department of the establishment appropriated to the sick. This forms an extensive infirmary, and is among the best clinical schools in the United States.

frame slender and emaciated, and with a temperament apparently cold and leucophlegmatic. The disease had existed in a greater or less degree for ten years. At the commencement, and long afterwards, she menstruated irregularly, as well to the time, as the quantity of the discharge, and always with pain and difficulty. But for the three last years, there had been an entire suppression. Still, however, she occasionally felt, once perhaps in two or three months, "as if nature were making an effort." At these times, she had for days together, fugitive pains in the back, and lower part of the abdomen, accompanied with the uneasy sensations of fulness in the chest and head.

As almost every other remedy had been employed by the physicians who had previously attended the woman, I did not hesitate to put her on the use of the Senega. I directed her to take of the decoction* a table-spoonful every two hours, and to live on a diet generous and nourishing. No immediate effect from this plan of treatment was discernible. In about three weeks, however, she confessed that she was better, both as regarded the acquisition of strength, and the general comfortableness of her feelings. Towards the expiration of the fifth week, I was requested, in the evening, to visit her. I found her complaining of very acute pain in the stomach like the colic. Her bowels being opened by an injection, she took a large dose of laudanum. In the morning, I learnt that she had been speedily

* In making the decoction, I put a pint of boiling water to an ounce of the Senega, bruised, in a close vessel, and let it slowly simmer over the fire till the quantity is reduced one third. Where the medicine excites nausea, which it is sometimes apt to do, even in small doses, I have it prepared with the addition of an aromatic, such as the orange peel or cassia. My rule, in the administration of the Senega, is to give about four ounces of the decoction, more or less, during the day, according to the circumstances of the case. But at the time, when the menstrual effort is expected to be made, and till the discharge is actually induced, I push the dose as far as the stomach will allow. I have given as much as two ounces every hour. In the intervals of the menstrual periods, I always lay aside the medicine for a week or two. Without these intermissions, if it does not lose its efficacy, it becomes nauseous and disgusting to the patient. While under a course of the Senega, the general system is to be kept properly regulated. Excessive excitement, or debility, is to be equally obviated by the use of the appropriate remedies.

relieved, by the anodyne, of the colic, but that she was at the moment, suffering from strong bearing down pains. She seemed also to be distressed by great restlessness and anxiety. As her pulse too, was somewhat full, and her skin warm, eight ounces of blood were drawn from her arm. The medicine was now increased to an ounce every hour. When I saw her again in the evening, no very sensible change had taken place. I prescribed the warm bath, and the continuation of the medicine. The next morning she was evidently better. The bath, the over night, had composed her; and after a few hours' sleep she began to menstruate. The discharge, however, was by no means natural. It looked like a mixture of the sanies of an unhealthy ulcer, and the fluid of leucorrhœa. During the day and succeeding night, the medicine was continued. Part of this time, the discharge was more abundant, and of a different appearance. It consisted of pretty tenacious mucus, so blended with grumous blood as to have a dark sanguineous complexion. The menstrual paroxysm now ceased. After a fortnight, she returned to the Senega, in the dose of an ounce four times a day, and persevered in thus taking it for three weeks, when she was again attacked. I was not called to see her till the third day of her illness. It appeared that she had been affected precisely in the same way as formerly: in the first instance being seized with colic, which was soon followed by bearing down pains. These lasted, with slight intermissions, for twenty-four hours, when a substance of an unusual aspect coming from her, I was sent for to examine it.* I found it to be a membranous lining of the uterus. It was exactly of the triangular figure of the inside of that organ, and of the dimensions of its cavity at the close of the second month of gestation. The external surface of it was shaggy or flocculose: the internal smooth. In consistence, it resembled firmly impacted mucus, or that structure denominated by anatomists *parenchyma*. In color, it was similar to the decidua, but twice or three times

* I ascertained that the Senega, in an increased dose, was the only medicine which had been given prior to my visit. No other remedy was afterwards employed during this attack.

as thick as I have ever seen that membrane.* The expulsion of this substance was neither preceded, nor followed, by hemorrhage, or other discharge, except a weeping of serum tinged with red blood.†

The woman, after a short interval, once more reverted to her medicine: and in the course of three weeks, I received the very satisfactory intelligence that she was *menstruating naturally*. Three cloths, which were shown to me on my visit to her, were thoroughly imbued with the genuine catamenia. Eighteen months have elapsed since this event, and she continues to do well, without any medical aid. The menses flow freely, but with no sort of periodical regularity. In every respect, however, her health has vastly improved.

I have traced the more minutely the history of the above case, because it strikes me as being interesting in several points of view. It at least corroborates an opinion which I had reason early to adopt, that in most, if not all the instances of obstinate and long continued amenorrhœa, the membrana decidua exists;‡ and that radical relief, under such circumstances, is to be alone sought in the riddance of this adventitious production.

The cases that follow, came under my care while in attendance at the Almshouse last winter. They will be more briefly related. Disappointed in procuring the records which are preserved, or ought to be preserved, in the clinical journal of the

* Morgagni was the first, as far as I know, who noticed this membranous substance. He describes it very accurately. By Denman our attention has been more recently called to the species of amenorrhœa to which it is incident. He mentions, among many other particulars relating to this substance, that he has known it to be generated, in more than one instance, by the virgin uterus. Such a case has never occurred to me.

† There is commonly in these cases, as much hemorrhage as in early abortions. It is said by Morgagni, that the expulsion of the membrane is succeeded by the lochial discharge. I have never observed it.

‡ Dr. Denman seems to entertain the same opinion. He, indeed, suspects that this membrane exists in every case of "habitually painful menstruation." There are two reasons why it is not more frequently observed. Women, in the first place, are reluctant to have their menstrual cloths inspected; and secondly, the membrane, in a great majority of instances, comes away in small pieces, or flakes, so that it may be overlooked, even when we have opportunities of an examination.

house, I am compelled to rely exclusively on my memory, which, at this interval, enables me to draw up only a general statement.*

CASE I.

The woman was a maniac. She had been confined to her cell for upwards of a year. During the greater part of this long period, she was furiously deranged. Of the cause of her insanity nothing could be ascertained. Her age appeared to be about twenty-five years.

I found that she had been most copiously depleted by the several physicians who preceded me. Bleeding, both general and topical, purging, blistering, and salivation, had all been pushed to a great extent. Besides these remedies, antimonial preparations, the digitalis, the decoction of tobacco, and a variety of *emmenagogues* had been tried. But, nevertheless, no very favorable impression had been made on her disease. Being told that she had not menstruated since she had entered the house, I determined, without delay, to endeavour to restore this function. I accordingly directed that she should take the decoction of Senega, in the accustomed dose; occasionally to lose some blood, and to use the tepid bath. After a few days her situation evidently meliorated; and at the end of some weeks, a flow of the catamenia came on, which was productive of the most salutary consequences. No morbid action now disturbed her system, and her mind shared in this new state of tranquillity. Encouraged by a change so propitious, the medicine was continued four or five weeks longer. Then menstruating a second time, she became still better, and her friends, considering her sufficiently recovered, took her home. Of late I have not heard of her: but for some time after leaving the house she had no relapse.†

* My friends Drs. Wallace and Morrice, of Virginia, who I believe took notes of these cases, will, I hope, correct, if they detect, any material inaccuracy in my statements.

† It may be proper for me to state, that though, when she quitted the house, she was cured of mania, yet her intellect, by the long derangement which it had suffered, was much impaired. It was, indeed, debilitated almost to fatuity. I understood, however, that her mind afterwards recovered somewhat of its tone.

CASE II.

This was also the case of a maniac. In many leading features it resembled the preceding one. The woman was about the same age, and her derangement of equal standing. In both instances the catamenia were suppressed. The treatment had been similar. There was this difference, however, in the state of the two women, on my first seeing them. The mind of the latter was far more sane. There was too, a strong tincture of lewdness in her aspect, her conversation, and deportment. This distempered ardor of appetite most rapidly increased. In less than a week she had confirmed nympho-mania, associated with extreme mental vivacity, and apparent joyousness of heart. Taking the whole of the circumstances of this case into consideration, I had little doubt as to the practice which ought to be pursued. My determination was promptly made up, to arouse the uterus, if possible, to a catamenial secretion. I was persuaded that a flow of the menses would most effectually quell this turbulence of passion, and probably likewise, as in the other case, alleviate her mental disorder. With the decoction of senega I therefore commenced. Of this she took, perhaps for a fortnight, without any visible advantage. The dose was in consequence augmented to two ounces every hour. At my next visit, three days afterwards, I was struck, on entering her cell, by a very extraordinary alteration in her appearance and demeanor. Instead of the eager and impassioned salutation which I had been in the habit of receiving from her, she remained on her bed tranquil and sedate. There was no longer any inordinate excitement. Composed both in body and mind, she seemed to be indulging a species of melancholy contemplation, from which it was not easy to divert her.

Inquiring of the nurse what had so suddenly revolutionized the condition of my patient, I was informed that she had been *menstruating copiously for the last thirty-six hours.*

As her health continued progressively to mend, I intrusted the case entirely to the powers of nature for two or three

weeks.* But I was constrained again to resort to the senega, in consequence of some menacing indications of a relapse. The medicine very quickly produced the anticipated effect. After menstruating freely, she became convalescent as before. My period of attendance in the house now expired, and she was consigned to my successor. What was the ultimate result of the case, I am unable to say.

To avoid prolixity, I shall dismiss these cases with one or two remarks only, though they are rich in practical reflections. Their importance is not restricted to the evidence which they bear to the powers of the senega as an Emmenagogue. They may, perhaps, open a new view in the treatment of mania in females. If they do not conclusively evince that amenorrhœa is sometimes a cause of insanity, they at least are calculated to awaken our suspicions upon the subject. Nor is this all. They show distinctly the intimate relation between the two diseases, and demonstrate, in the most striking manner, how essential it is in treating the affections of the mind, to have a constant and vigilant eye directed to the uterine function. They prove, in short, incontestably, that in some cases of mania in women, a cure may be effected, when other means have totally failed, by simply producing an effusion of the menses.

It now only remains for me, very concisely, to suggest the mode in which I conceive the senega to operate. Inquiries of this nature are not without utility. They lead to the establishment of principles, by which we regulate our practice with greater precision and certainty.

The menstrual discharge I hold to be *the result of a secretory action of the uterus*. Every other hypothesis on the subject is totally irreconcilable with facts, and repugnant to the laws of the animal economy. The crude speculations of former times respecting this operation may, indeed, be considered as discarded. Does any one, for instance, whose knowledge has kept pace with the improvements of physiology, now entertain the notion of lunar influence, of fermentation, of venereal ap-

* Since writing the above, I recollect that blisters, at different times, were applied both to the inside of her thighs, and ankles, and kept open during the greater part of this period.

petite, of general plethora, or local congestion, &c. &c.? The theory of secretion I am aware is not fully established. But much may be urged in its support. My limits, however, will permit me at present, to bring to its defence merely a summary of the more leading arguments. These may be thus enumerated.*

1. That the uterus in its villous, and vascular structure, resembles a gland, and also in its diseases, being equally liable to scirrhus, cancer, &c. &c.

2. That, like other secretory organs, blood is very copiously diffused through it.

3. That, by the arrangement of its vessels, it is evidently designed that the circulation should be retarded for the *purpose of secretion*. The arteries are not only exceedingly convoluted, but they are larger and with thinner coats than their corresponding veins.†

4. That in common with other secretions, menstruation is often, at first, imperfectly performed, and is subject afterwards to vitiation and derangement. In the beginning the discharge is commonly thin, colorless and deficient, and recurs at protracted and irregular intervals. In some of these particulars, it is analogous to the seminal secretion.

* Who originally suggested the theory of secretion, I have not been able to ascertain. It has very generally been ascribed to the celebrated Mr. Hunter; but the evidence of his claims to it is exceedingly slender. The only trace of it, which I can discover in his writings, is a vague expression in a paragraph of his Treatise on the Blood. Afterwards, however, he furnished an extract from his lectures to be published in Johnson's Midwifery, as exhibiting more fully his notions respecting this function. Speaking of the death of the blood from lightning, and other sudden causes, he includes the catamenia among the illustrations of his reasonings. "The blood," says he, "discharged in menstruation, is neither similar to blood taken from a vein of the same person, nor to that extravasated by an accident in any other part of the body: but is a species of blood changed, separated, or thrown off from the common mass by an action of the vessels of the uterus, in a process *similar to secretion*, by which action the blood having lost its living principle does not coagulate," &c. I am told that the theory occurs in an old French system of physiology. It was certainly published in a thesis, at Edinburgh, by Dr. Craven, in the year 1778.

† "The blood," says Haller, "is brought to the womb in greater quantity, and more quickly through its lax and ample arteries, and on account of the rigidity and narrowness of the veins, it *returns with difficulty*."

5. That in many of the inferior animals, during the season of venereal incalescence, there is an uterine effusion which is undoubtedly *a secretion*. This answers seemingly the same end as menstruation, namely, giving to the uterus an *aptitude to conception*. Though this fluid generally differs from the menses in complexion, yet in some instances, they are precisely similar. Whenever the venereal desire suffers a violent exacerbation from restraint, or other causes, the discharge in these animals becomes red. This has been more especially remarked in bitches kept from the male.

6. That when the menses are suppressed, they cannot be restored by inducing plethora, nor the flow be checked by blood-letting, or any other means of depletion. Besides, no vicarious discharge relieves the symptoms of suppression! Do not these facts very unequivocally proclaim the existence of a secretory function?

Lastly. That the menses are a fluid *sui generis*, or at least varying very essentially from blood; having neither its *color*, nor *odor*, nor *coagulability*, and on chemical analysis present *different results*. Let me ask, if the menstrual fluid be *not blood*, what is it?

To the objection, which has sometimes been urged, that the uterus is not sufficiently glandular for the office alleged, it may be, I think, very satisfactorily replied, that there is hardly a viscus, or surface of the body, which is not competent to this purpose. It would really seem that no operation of the animal economy requires a less complex apparatus. Of what, indeed, does a gland consist, except a congeries of vessels? Even the most perfect of the secretions are accomplished by this simple contrivance. If a few vessels, "creeping through the coats of the stomach," can secrete the gastric liquor, why may not the infinitely more glandular organization of the uterus elaborate the menstrual fluid?*

* As yet we know of no glandular structure in vegetables. They contain only tubes or vessels, through which the fluids circulate. Notwithstanding, however, the want of glands, we find the sap of plants converted into oil, mucilage, acids, &c. No stronger proof can certainly be required of the extreme simplicity of the organs by which the secretory transformations are effected.

By admitting the truth of my position, we have, at once, a solution of the problem before us. It follows necessarily from the concession, that amenorrhœa is caused by an interruption to the secretory action of the uterus; and that the remedies which remove the suppression, operate by bringing the organ back to that precise condition, on which the exercise of this function depends.

No one, who has carefully watched the effects of the medicine upon the system, but must have perceived how actively it promotes the various secretory discharges. This is especially manifested in the glands of the mouth and fauces, and the neighbouring secretory surfaces. Excepting the mercurial preparations, I doubt exceedingly whether we have any medicine so eminently possessed of the property to which I allude. Unquestionably there is none that so uniformly excites ptyalism.

Deny to the senega the specific energies for which I contend, and where shall we seek an explanation of its effects? If it be simply a stimulant, or tonic, or sudorific, as is more generally supposed, it might kindle excitement, or impart tone, or raise a diaphoresis, like many other articles of the materia medica; but would it be so signally efficacious as an Emmenagogue?

The doctrine of *specific actions* is not new. It once prevailed in the schools of physic, but has been supplanted by hypotheses which are, in my opinion, far more remote from the truth. The bold assumption that all medicines are similar in their operation, differing only in degree of force, permanency, and diffusibility, however plausible in speculation, will not bear the test of facts. The directly opposite conclusion is abundantly confirmed by experience.

I believe, that scarcely any two agents produce precisely the same effects upon the living system. Hence the *variety of diseases*, and the necessity of a *multitude of remedies*. Not the slightest of the causes, which conspire to retard the progress of physic, is the eagerness to generalization, that at all times has distinguished its cultivators. Of late, however, this propensity has been carried to the utmost extremity. Disease is now held by an eminent teacher to be *a unit;* and all medicines are considered, by the disciples of another, as "participant of one effect." Such hypotheses may have the semblance of the simpli-

city which they claim, but, in *reality*, they are "confusion worse confounded."

If we are ever to strip our art of its "glorious uncertainties," and to introduce into the practice of it something of exactness, it will be by pursuing a very different course. To accomplish so important a revolution, we must diligently, and without prepossessions, observe the *phenomena of disease*, and, with an attention no less unbiassed, investigate the *properties of medicines*. Thus, perhaps, we shall ultimately learn to distinguish accurately the *diversified shades* of morbid action, and to apply to each its *appropriate* remedy. As it is, however, at present, we are plunged into a Dædalian labyrinth without a clew. Dark and perplexed, our devious career, to borrow the fine illustration of a favourite writer, resembles the blind gropings of Homer's Cyclops round his cave

But to return from this digression. In all the forms of amenorrhœa, I suspect the senega may be useful, if administered with a proper regard to the state of the system, and in other respects, with correct discrimination. But it promises to be more particularly so in those cases where the decidua exists. We are, as yet, ignorant of the exact process by which the membrane is framed. Of this, however, there is no doubt, that the vessels of the uterus, which pour out the catamenia, are the instruments by which it is done. Nor is it less certain, that while they are engaged either in the formation or support of this new production, menstruation ceases. The two offices, indeed, exact modes of action totally incompatible. It is obvious, that under these circumstances, to excite the uterus to a secretory effort, not only a *forcible*, but a *specific impression* must be made upon it. I am entirely persuaded, both from my speculative views of its properties, and my actual experience, that the powers of the senega will be here most conspicuously displayed. Should my sanguine expectations of its efficacy in these cases be fully confirmed, it will indeed be an important acquisition. This species of amenorrhœa is, on many accounts, so serious an evil, and has hitherto proved so difficult of cure, that a remedy for it is, confessedly, among those things the most anxiously desiderated in the practice of physic.

Philadelphid, September 20, 1811.

P. S. I received the annexed communication after my own paper was committed to the press. I am glad to have it in my power to furnish such additional evidence, and from so respectable authority, to the efficacy of the senega.

Dear Doctor,

I send you an account of my experience in the use of the senega polygala in suppression of the menses, to Dec. 1809. Since that period I have frequently prescribed it, but have not kept accurate notes of its effects.

My confidence in the efficacy of this medicine is by no means diminished.

Respectfully, &c.

J. HARTSHORNE.

Dr. N. Chapman.

I have used a saturated decoction of the senega in twenty-three cases of amenorrhœa. In thirteen of these, the menses appeared during the use of the decoction; in two, the health of the patients was very much improved, but the catamenia did not return; in three, no advantage whatever was derived from it. In the remaining five cases, I have not been able to ascertain the event. In the last case in which I used this remedy, the chlorotic symptoms were very violent. The menses had been suppressed eight months. From twelve to sixteen ounces of the decoction were taken in the course of twenty-four hours, for two weeks; the menses then appeared, and the patient has since enjoyed much better health.

The senega has sometimes induced a flow of the menses, without alleviating the disease which accompanied the suppression.

I have always wished to commence the exhibition of this remedy at least two weeks before the period when the patient had usually menstruated. It produces no mischievous effects, and might perhaps be exhibited with safety for a much greater length of time. My experience however does not warrant an assertion that its long continued use would not be injurious, as none of my patients have continued it more than two weeks.

An Analysis of the Water of the Sulphur Springs, situated in Adams County, Pennsylvania.

BY JAMES CUTBUSH,

Lecturer on Chemistry, Philadelphia.

The following experiments were made with the water brought from the spring by Mr. John Greiner, which was bottled, corked, and bladdered.

1. *Examination by Reagents.*

Exp. 1. Alcohol of galls did not produce a change, nor did prussiate of potash, nor succinate of ammonia.

2. Muriate, as well as acetite of barytes, produced a precipitate insoluble in muriatic acid.

3. Lime water produced no change.

4. Nitrate of silver gave a white precipitate.

5. Solution of soap occasioned a turbidness.

6. Oxalate of potash produced a precipitate.

7. Caustic potash gave a white precipitate soluble in muriatic acid.

8. Carbonate of potash produced a precipitate soluble with effervescence in muriatic acid.

9. Carbonate of ammonia was added to a portion of the water; the liquor was then filtered; and phosphate of soda, added to the clear liquor, gave a copious precipitate.

10. Sulphated silver gave a white precipitate.

The hepatic smell, of which the water partakes at the spring, was entirely lost by the conveyance; but, on examining the earthy matter taken from the bottom of the spring, which Mr. G. also brought, evident signs of sulphur in an uncombined state were discovered. The experiments already described prove the presence of several substances, namely:

Sulphuric acid, muriatic acid, lime, magnesia.

Exp. 1. proved the nonexistence of iron.

2. The presence of sulphuric acid.

3. The nonexistence of carbonic acid.

4. The presence of muriatic acid.

5. The existence of earthy salt.
6. The presence of lime.
7. The existence of earthy matter.
8. Do. do.
9. The presence of magnesia.
10. This experiment confirmed experiment 4.

Having thus pointed out, in a general manner, the fixed ingredients of the York spring, the next object of research was to determine the proportion of the magnesian and calcareous salts, and to ascertain if the muriate, which was detected, was of an earthy or an alkaline nature. The following section of experiments was therefore instituted.

II. *Examination by Evaporation, &c.*

Exp. 1. Two ounces, by measure, of the water was evaporated to dryness: the residue was carefully collected and weighed. It amounted to four grains. One pint, or sixteen ounces, therefore contains thirty-two grains.

2. To a portion of this powder, a few drops of sulphuric acid was added: muriatic acid gas was disengaged.

3. In order to ascertain the presence of muriate of soda, a portion of the water was evaporated until a film formed on the surface. The vessel was then removed to a cool situation. Small cubic crystals now appeared.

These experiments prove, that in sixteen ounces of the water, thirty-two grains of saline matter exists; and that it is composed of sulphate of magnesia, sulphate of lime, and muriate of soda. To ascertain the relative quantities of each, a number of experiments were instituted. The formula adopted for this purpose was agreeable to Klaproth. After separating the muriate of soda, the remaining portion, consisting of the two earthy salts, was treated with twice its weight of cold distilled water. The insoluble part was collected on the filter, and washed with a small quantity of water; it was then dried in a low red heat, and weighed. After thus separating the sulphate of lime, the soluble part, consisting of the magnesian salt, was treated with carbonate of potash; the precipitate (carbonate of magnesia) was washed, dried, and calcined.

Accordingly I found, that thirty-two grains of the saline matter contained

Sulphate of magnesia, (Epsom salt)	20
Sulphate of lime, (gypsum)	6
Muriate of soda, (common salt)	4
Loss,	2
	32

Or, very nearly,

Magnesia,	11
Lime,	3
Sulphuric acid,	12 grains.

Consequently, $11+3+12+4=30$ grains; or the proportion of the ingredients, according to experiment, in one pint of the water.

The water at the spring, I am informed, is strongly hepatic; but in consequence of the carriage, it was entirely lost. Mr. Greiner, at the same time, favoured me with the earthy matter, said to contain sulphur, obtained from the bottom of the spring. On examining it, I found it strongly impregnated; for, on exposing it to ignited coals, it emitted a strong smell of sulphur. I judge from this phenomenon, and from the nature of the water, that sulphur exists in the neighbourhood, in union with earthy substances; and on their decomposition, no doubt, depends the formation of the sulphates already spoken of.

When this matter is taken from the spring, it is of a black color; but when it becomes dry, the color changes to a darkish gray. It is said to be useful in herpetic eruptions.

Hints on the Treatment of Insane Persons.

For the Eclectic Repertory.

THE following general hints on the treatment of insane persons have been chiefly drawn from the experience of *The Retreat*, near York (Eng.) The compiler however is fully aware, that after all which can be learnt from others, much must remain for experience to teach. The modes of insanity, and consequently its treatment, will be found to vary, in no small degree, with all the varying characters, manners, and habits of the human mind; and therefore the object, to which those concerned in the establishment of lunatic institutions ought most peculiarly to direct their attention, is the selection of a manager who possesses, in addition to other qualifications, a natural quickness of perception, and tendency to observe the varieties of mind, which will enable him to perceive with readiness, and apply with address, such moral treatment as the different cases may require.

Medical Treatment. It must be confessed, that the experience of the Retreat has not thrown much light on the medical treatment of insanity. It has however led the manager to believe, that but little is to be done by its aid, and convinced him of the folly as well as cruelty of forcing upon the patient a large quantity of nauseous draughts, at a time when probably his aversions are more than usually strong, and when he is with difficulty induced to take the food necessary for his support. The use of medicine is however far from abandoned at the Retreat. In all cases where the disease has supervened, or is attended by any obvious bodily disease, however slight, advantage may be reasonably expected from the removal of such complaint.

The warm bath has been much used at the Retreat for several years in all cases of *melancholy*, with the happiest effects. Indeed there has not been any recent case in which it has been employed without relieving or removing the complaint. The patient usually makes use of the bath every other day, and continues in it about twenty minutes, at a temperature of about

eighty-five degrees. The time of continuance is gradually advanced to nearly an hour, and the temperature to about ninety-six degrees.

The difficulty of obtaining sleep for maniacal patients, and that opium, though taken in large doses, frequently produces no effect, is well known. It suggested itself, however, to the sensible mind of the superintendent, that all animals in a natural state, repose after a full meal, and, reasoning by analogy, he was led to imagine, that a liberal supper would perhaps prove the most effectual anodyne. He therefore caused a patient, whose violent excitement of mind indisposed him to sleep, to be supplied freely with meat, or cheese and bread, and good porter. The effect answered his expectation, and this mode of obtaining sleep has since been generally successfully employed. In cases where the patient is averse to take food, porter alone has been employed with great advantage. Medical attention to the general bodily health, during convalescence, and in the lucid intervals of the patient, is of great importance. The return of the disease may frequently be suspended, if not prevented, by this kind of attention. The operation of cupping, applied to the head, temples, or neighbouring parts, has very frequently been attended with great advantage, where any pain or heat has been felt in the head. General bleeding and other evacuations have been found injurious at the Retreat, and are therefore not used, except where their necessity is indicated by the state of the bodily habit.

Air and Regimen. The situation of the Retreat is high, and affords a very dry air, which there is reason to believe is of very great importance to lunatics, but more especially for those of the melancholy class.

All the patients should, as much as their cases will allow, be exposed to the open air, and particular care should be taken that their different apartments be well ventilated.

A cheerful, pleasant scenery, in a retired situation, seems a desirable appendage to a dry and healty site for the building. A *single* range of rooms, with a gallery opening to the external view, is much preferable to a range of rooms on each side of a gallery. For though the exclusion of much light, in some violent cases, is found desirable, this is not a general circumstance.

Every *appearance* of a place of confinement should be studiously avoided. The window frames at the Retreat are cast iron sashes of a full size, with panes too small to admit of a patient's escape through them. Separate wards for the men and women patients, and the different descriptions of each sex, are also very essential.

The regimen of lunatics should be regulated by a consideration of the nature of the disease, and the state of the bodily habit. The experience of the Retreat has fully convinced the manager of the general impropriety of reducing the patient, however violent, by a low diet, or violent evacuations.

The following is the general diet of the patients, viz.

Breakfast. Milk and bread, or milk porridge.

Dinner. Pudding and flesh meat six days in the week; fruit, pudding, and broth, one day.

In the afternoon. The men have bread and beer, the women tea or coffee.

Supper is generally the same as breakfast; sometimes bread, cheese, and beer.

The parlour patients, when sufficiently well, partake of whatever comes to the superintendent's table, and some are supplied from it in their own apartments.

Moral Treatment. It will naturally occur to most persons, that the first objects to which the attendants upon lunatics ought to apply, is to obtain their entire confidence and good opinion, and for this purpose, it is necessary to treat them with uniform kindness, and never to deceive them, either by promises or threats. The idea, which has too much prevailed, that it is necessary to commence an acquaintance with lunatics by an exhibition of strength, or an appearance of austerity, has been found, at the Retreat, extremely erroneous, and to be a part of that cruel system, dictated perhaps by timidity, which has so long prevailed, and unhappily still prevails in many of the receptacles for the insane. Perhaps, in general, much familiarity, when a patient first enters the house, would have a tendency to lessen that authority which it is in some cases necessary for the attendant to possess and exert; but, let it be remembered, that in most instances of mental depression, the behaviour, though it ought to be firm, cannot be too kind, con-

ciliating, and tender. There may, however, be particular cases in which the attendant may, perhaps with advantage, assume a distant and somewhat important manner, but it must be done with extreme judgment, as the observation of maniacs is frequently morbidly acute.

It rarely happens that all the faculties are deranged at the same time, and the moderate exertion of those which remain sane, is calculated to correct and strengthen those which are diseased. The patient on all occasions should be spoken to and treated as much in the manner of a rational being as the state of his mind will possibly allow. By this means, the spark of reason will be cherished, and that painful feeling of degradation, which must be felt in a greater or less degree by all who recover the loss of their rational powers, and which cannot fail materially to depress them, will be greatly lessened. During the state of convalescence, attention to this hint is peculiarly important, and the greatest advantage has been found to arise at the Retreat, from introducing the patients who appear to be recovering, into the society of the superintendent and the other rational parts of the family. This liberty is afforded to the poor as well as the more opulent patients, as it has been found very materially to accelerate the recovery, if indeed it be not essential to it.

Of the modes of employing the minds of maniacs, those are to be preferred, which are accompanied by bodily action, which are of the amusing kind, and are most opposite to the illusions of their disease. When the patient is himself inclined to any particular employment, if not very unsuitable, it should be seized as a favourable circumstance, and carefully encouraged.

Conversation with those who can condescend to their weaknesses, and walks into the country, under suitable care have been found of essential benefit to the different classes of patients. The salutary effects of air and exercise, the variety and beauty of the objects of nature and rural life, as well as the subjects of conversation which these excite, have a strong tendency to still the effervescence of an overheated imagination, and to inspire the anxious and melancholy mind with tranquil and pleasing emotions. The experience of the Retreat has fully shown that, in melancholic and hypochondriac cases, close

confinement is of all things the most unsuitable. Hence we may explain, why so few cases of melancholy are cured in lunatic asylums in general, and hence, happily, we may also, in great measure explain, why so many labouring under these most affecting complaints have been restored, by their treatment at the Retreat, to their families and society.

Several of the convalescents are made useful in assisting the attendants, and the females in general are employed in needle-work, where they can be safely intrusted with the necessary implements. The use of books and pen and ink are generally allowed, and the indulgence has been found generally beneficial.

It must however be allowed, that a suitable mode of employing the men patients is still a desideratum.

The principle of honour is often very strong in the minds of lunatics. I have known patients, who were under a voluntary engagement of good behaviour, hold a successful contest, for a considerable length of time, with the strong wayward propensities of their disorder, and even conceal all marks of aberration of mind. The attempt is highly beneficial to the patient, and ought to be sedulously encouraged by the attendant.

Several of the patients are permitted to attend the meetings in the city, and all who are suitable are assembled together on a first day afternoon, when the superintendent reads to them some chapters of the Bible. A profound silence ensues, during which, as well as the reading, it is curious to observe the order with which the patients conduct themselves, and control their different propensities. In cases where the patient is disposed to be violent, advice given in a friendly manner is very often successful in preventing the necessity of harsh measures, which ought never to be employed without absolute necessity, and then with obvious marks of regret on the part of the attendant. It is proper however to observe, that no advantage has been found to result from reasoning with maniacs on their particular hallucination. One of the distinguishing marks of insanity is a false conception, which of course occasions an incapacity of conviction. The attempt therefore generally irritates the patient, and rivets the false perception more strongly on his mind.

When a patient proves refractory, and coercive measures are indispensable, it is advisable to have an ample force em-

ployed, as it prevents in general any attempt at resistance; but where such force cannot be obtained, and the case is urgent, courage and confidence will generally overcome the violence of the patient, for there are hardly any instances in which maniacs have displayed true courage. The superintendent of the Retreat was one day walking, in a field adjacent to the house, with a patient who, when opposed, was apt to be vindictive. An exciting circumstance occurred; the maniac retired a few paces, and took up a large stone, which he held up as in the act of throwing at his companion. The superintendent, in no degree ruffled, fixed his eye upon him, and in a resolute tone of voice, at the same time marching towards him, commanded him to lay down the stone. As he approached, the hand of the lunatic gradually sunk from its threatening position, and dropped the stone to the ground. He then submitted to be quietly led to his apartment. One motion of timidity on the part of the superintendent might have cost him his life.

Modes of coercion. Kind yet firm treatment has been found to supersede the necessity of much coercion of any kind. Neither chains nor corporal punishments have *ever* been allowed on any pretext at the Retreat. The strait jacket, or a belt round the waist, which has straps that confine the arms close to the side, are the only instruments of coercion made use of when the patient is not in bed; when it is found necessary to confine the patient in a recumbent posture, the superintendent has invented a mode of fastening, which allows a change of posture, and the bending of all the joints of the body.

If in any instances chains are necessary for the confinement of lunatics, it is much to be feared that the highest pitch of maniacal fury has been excited by the cruelty or improper treatment of the attendants, very few of whom are fit to be intrusted with much power. It should therefore be as limited as it can be with safety, that they may be obliged to use every means, to govern rather by esteem than severity, and avoid every thing likely to exasperate the patients.

Inguinal Aneurism cured by tying the External Iliac Artery in the Pelvis.

BY JOHN SYNG DORSEY, M. D.

For the Eclectic Repertory.

On the 15th of August 1811, I was consulted by Alexander Patton, on account of a tumor in his right groin. The patient was a native of Aberdeenshire in Scotland, aged about thirty years, the last ten of which he had passed in America, having followed the trade of a cooper; was accustomed to hard labour, and to athletic exercises, jumping, running and the like. He was six feet in height, of a robust but not a corpulent habit.

Two years ago he perceived, for the first time, a small tumor in the right groin. Having never had the venereal, nor indeed any other disease, and not having met with any accident, he was at a loss to account for this appearance. From its commencement it throbbed with considerable violence. For a year and four months it increased very slowly; during the last eight months much more rapidly. In January it was no bigger than a walnut; in August its shortest diameter was four inches, its longest, nearly five. It occasionally gave him severe pain, and at length incapacitated him from all labour. In June last he applied to Dr. Irwin of Easton, the place of his residence, who instantly apprised him of the nature and importance of the complaint, and advised him to come to Philadelphia. He arrived here the 14th of August, and was admitted next day into the Pennsylvania hospital.

On examination, an aneurism was found, situated immediately below Poupart's ligament, forming a regular tumor in the groin, nearly hemispherical, with a kind of apex, where the skin appeared extremely thin, and discolored as if by ecchymosis. The patient had used a good deal of exercise previously to his admission into the hospital, and had taken a drink of rum, in consequence of which his arterial system was greatly excited, and the tumor pulsated so violently that the bed clothes were bounced up with great force. He was confined to bed; was purged, and kept to a low diet. A consultation was called;

and the surgeons of the house concurred in recommending the operation of tying the artery as high as practicable above the tumor. It was determined to perform the operation promptly; as the disease was progressing, and no benefit was to be expected from delay.

On Monday, 19th of August, at noon, in presence of Dr. Physick and Dr. Hartshorn, surgeons to the hospital, and a number of medical gentlemen, I proceeded to the operation. The patient, having previously taken fifty drops of laudanum, was placed on the table. An incision, three inches and a half long, was made, beginning an inch and a half higher than the superior anterior spinous process of the ilium, and one inch distant from that process internally; being also four inches and a half distant from the umbilicus, extending obliquely downward, and terminating about one inch above the basis of the tumor. This incision, which was nearly in the direction of the fibres of the tendon of the external oblique muscle, divided the skin and adipose membrane, and exposed that tendon, which was next cut through, the whole length of the external incision. The internal oblique muscle now protruded at the wound and was carefully cut through; the inferior edge of the transversalis abdominis was next divided, but not so far upward as the top of the external wound. My finger was then introduced, and the cellular texture readily yielded it a passage to the external iliac artery, the trunk of which I distinctly felt pulsating very strongly. With my finger I separated it gently from the neighbouring parts; but took care to denude only a very small portion of the vessel. The peritoneum I was equally careful to detach as little as possible; and not more than a square inch of it was disturbed. The only remaining difficulty in the operation was to pass the ligature round the vessel; and this having been anticipated, was readily surmounted. Before commencing the operation, I had secured an aneurismal needle (a blunt bodkin of silver properly bent) in a pair of curved forceps, by tying the handles of the forceps firmly together. The needle was armed with strong bobbin; and thus connected with the forceps, resembled a tenaculum, which could easily be managed outside of the wound. With one finger in the wound I found it very

easy to direct the extremity of the needle, and with the forceps in my other hand, to push it through the fascia surrounding the vessel. The string connecting the handles of the forceps was now cut, and the needle was left under the vessel. The forceps being removed, the needle was drawn out, leaving the ligature round the artery. Convinced, by careful examination, that nothing but the artery was included in the ligature, and that it was, to the best of my judgment, natural in size and texture, I tied it very firmly, as high up as possible. The pulsation of the tumor instantly ceased. Three knots were made, and the ends of the ligature were left out at the external wound. No bloodvessel of magnitude was divided, and not half an ounce of blood was lost. No stitches were employed to close the wound; a strip of adhesive plaster effectually answered this purpose. A pledget of lint was applied, and the patient was put to bed, his thigh being moderately flexed upon the pelvis. He complained of extreme pain during the latter part of the operation, the whole of which occupied eleven minutes.

The patient's pulse, for several days before the operation, was 80: after the operation it was 88, and rose in the afternoon to 100. At four o'clock he was bled ten ounces. At seven he complained of extreme pain in the back and belly, and also of some pain in the limb. He was not permitted to take any sustenance except toast and water. The superficial veins of the leg and foot were filled; and the whole of the limb was covered all the evening with perspiration. Its temperature was examined repeatedly by a thermometer, and was five degrees colder than the other. It was covered with flannel and carded wool.

Tuesday 20th. Passed a restless night, in great pain. To use his own language, in expressing his sensations, "he felt as "if his loins were tearing apart." He was also troubled with pain of the bowels. Three grains of calomel and ten of rhubarb were given, but without procuring a stool. In the afternoon he was bled ten ounces, and a purgative injection was ordered; after which his bowels were freely opened, and his pain subsided. An enema, consisting of a hundred drops of laudanum and two ounces of water, was administered, and he soon after

fell asleep. The weather, on the day of the operation and several days after, was very hot. The mercury of the thermometer in the patient's room stood at 86° Fahrenheit. Placed between the toes of the aneurismal limb, it rose to 88°; between those of the sound limb 90°; at both knees it stood at 92°. His pulse was 100 and tense.

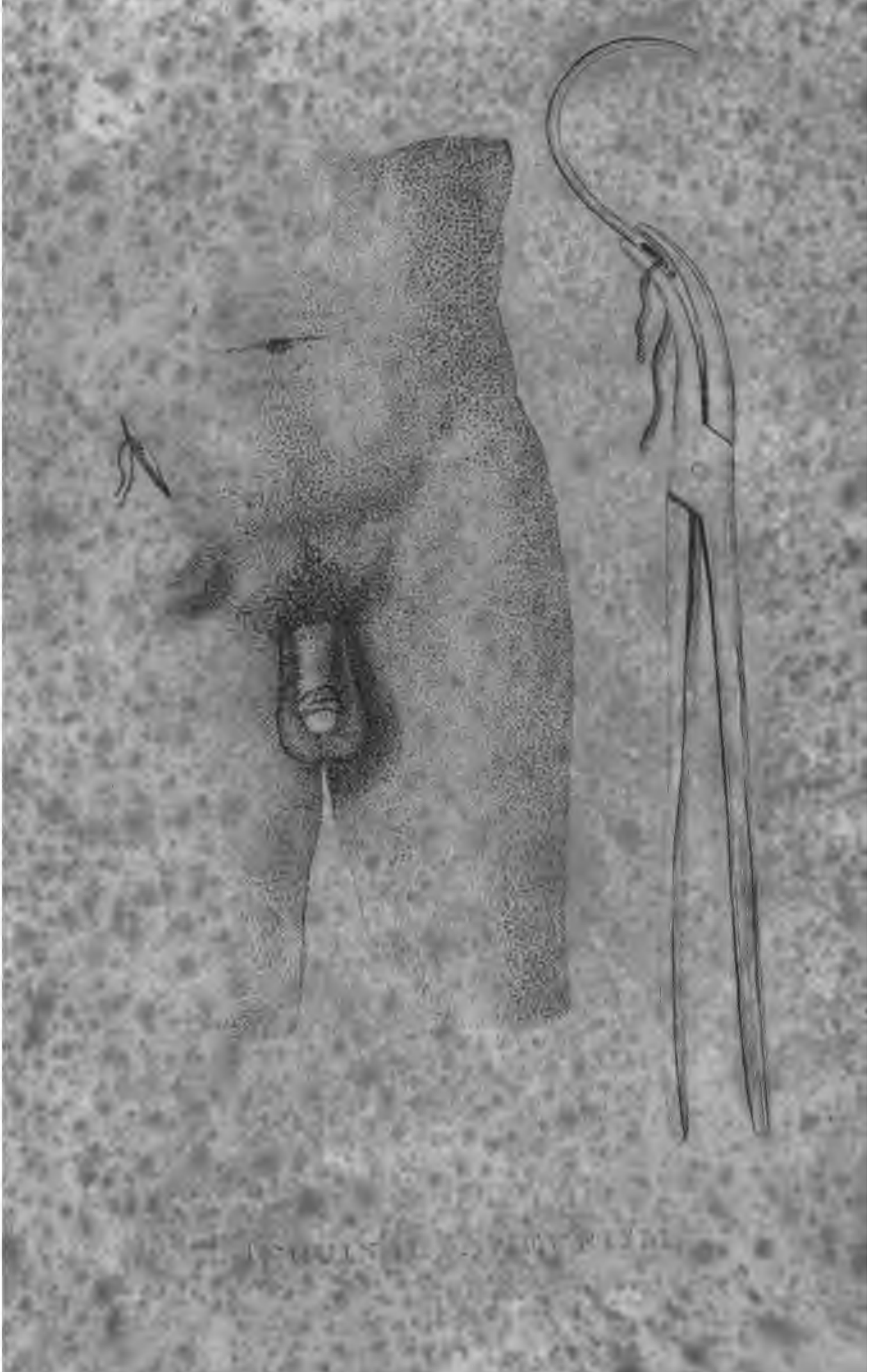
Wednesday 21st, *third day* after the operation. The sleep procured by the anodyne injection continued all night. In the fore part of the day he was easy; but in the evening his pain returned with considerable fever. He was bled ten ounces and took ten grains of magnesia and as much rhubarb: this with the assistance of a clyster, brought away a large quantity of fæces and flatus, and procured relief of all his pain. The anodyne injection was again administered, and he soon after slept. His pulse 100 and somewhat tense.

Thursday, *fourth day*. He slept all night, and was much better; being quite free from pain and fever. His pulse 90. He ate some boiled rice with great relish. The wound was examined, and it was found that nearly all of it had united: a little healthy pus surrounded the ligature. The limb was four degrees colder than the sound one.

From this time no change of importance occurred until Sunday, 1st Sept. when the ligature came away; viz. on the fourteenth day after the operation. In a few days more the wound cicatrized, without the occurrence of a single unpleasant symptom. On the twentieth day after the operation, his nurse being absent, he arose from bed and walked across the room, and has taken exercise every day since without inconvenience. The tumor in the groin diminishes slowly, and at this time is much reduced in size.

Remarks. The operation of tying up the external iliac artery above Poupart's ligament was first performed by Mr. Abernethy, under circumstances in which immediate death was the only alternative. He repeated it afterwards in cases of aneurism seated so high in the femoral artery as to preclude all prospect of a cure by any other means. He performed the operation four times. In the first two instances his patients

* Abernethy's Surgical Observations



died; in the succeeding cases they recovered.* Mr. Freer, in the Birmingham hospital, performed the operation in a case of inguinal aneurism with complete success; and soon after another case was treated successfully by Mr. Tomlinson, in the same hospital.† These six cases are all I have seen related, in which the operation has been tried. The case I have now detailed is the seventh; and it has failed in only two of these. In every instance the limb has been supplied with blood, which does not uniformly happen after the operation for popliteal aneurism.

I wish, before closing this paper, to call the attention of those surgeons, who may have occasion to perform the operation, to the forceps, an engraving of which is annexed. This instrument was contrived several years ago by Dr. Physick for the purpose of passing a needle under the pudic artery, when wounded in lithotomy, and has since been used by him for securing bleeding arteries in deep narrow wounds. Mr. Abernethy complains of "the great difficulty of turning a common needle in a deep narrow wound;" and Mr. Freer was unable to pass his aneurismal needle round the iliac artery, until he punctured the fascia surrounding it with his knife, which he confesses was the most "difficult and dangerous part of the operation." These dangers and difficulties are entirely obviated by means of the curved forceps; and I think the operation greatly simplified by the use of this contrivance.

Should this paper meet the eyes of Mr. Abernethy, I hope he will be gratified with the additional testimony of the importance of an operation for which the world is indebted to the intrepid efforts of true genius; and he will no doubt learn with pleasure, that one individual on this side the Atlantic owes to it, his life.

Arch Street, 30th September, 1811.

☞ *The sketch which accompanies this communication was intended to point out the part, where the operation was performed, and the instrument used to convey the ligature round the artery. I needs no particular explanation.*

† Freer on Aneurism.

ORIGINAL REVIEW.

On the Morbid Sensibility of the Eye, commonly called Weakness of Sight. By John Stevenson, member of the Royal College of Surgeons, &c. London, 1810,

THE author informs us, in the introduction, that he has been frequently consulted by persons afflicted with this complaint, which has not, he believes, been hitherto described as a distinct disease, nor has its cure been established upon a rational pathology.

Many of these cases had been of long continuance, and had been treated with tonic remedies, on the supposition that the disease proceeded from debility. The author was for some time of this opinion; but the failure of success induced him to change his sentiments and practice. The following is his description of the disease.

By simply inspecting the eye, it is scarcely possible to recognise the existence of this complaint; as there is not the slightest external ophthalmia or unusual fulness of the vessels of the conjunctiva, any apparent affection of the ciliary glands, nor the least visible organic derangement. The characteristic symptoms are, a morbid sensibility of the eye to light, and to different kinds of external stimuli. A strong glare of light is always painfully distressing to the patient; and hence, in aggravated cases of this disease, the effulgence of the sun's rays, when admitted to the eye, excites in it a very acute sensation, which is accurately referred to the bottom of the orbit; around which there is at the same time, a sense of tension, and of oppressive uneasiness. For the same reason, the patient is miserably uncomfortable in a brilliantly illuminated apartment. In order therefore to exclude the strong and direct rays of light, he instinctively depresses his eyebrows, or applies his hand to his forehead, viewing objects with the palpebræ half closed; by which he is apt to acquire the habit of blinking. If he attempts to read, or look at small or bright objects, he is soon dazzled, and his vision becomes confused; which, added to the pain the effort occasions, speedily compels him to desist.

The iris acts with great energy on the admission of the rays of light to the retina, and, in consequence, the pupil becomes contracted to a very small aperture; a STRIKING FEATURE of this disease. When the stimulus of light affects the eye, there is sometimes, though very rarely, a manifestly deficient action of the lachrymal glands; but much oftener the secretion of tears is abundantly copious, which is indeed the principal cause of the confused vision occasionally attending this malady.

General debility, however induced, though not essential, seems greatly to *predispose* to this complaint. Hence its most frequent occurrence to persons recovering from previous illness, and to those of a relaxed habit. Although I have witnessed it in both sexes, yet the female, for the causes just specified, seems most obnoxious to its attacks; which may take place at all periods of life. Preceding ophthalmia, whether acute or chronic, gives the organ a great susceptibility to this disease; which is very apt to recur on the application of the exciting causes. These are undoubtedly, the long or frequent exposure of the eye to a very vivid or reflected light, and its excessive exertion in reading or viewing minute and dazzling objects. And with regard to the proximate cause of weakness of sight, instead of local debility, I will hazard the opinion, that it consists in an exquisite irritability and sensibility of the retina, the effect of a great turgescency of the vessels, or a chronic inflammation of that membrane or of the choroid.

Agreeably to this supposition, the indications of cure must consist, not in giving additional tone by the use of cold astringent applications, and internal strengthening medicines, but in lessening the plethoric state of the vessels of the posterior membranes of the eye, and in obviating, at the same time, the extreme sensibility of the retina.

The author accordingly directs the application of leeches to the lower eyelids; a few grains of calomel at bed time, to be purged off next morning with a solution of cream of tartar and emetic tartar; to foment the eyes morning and evening with an infusion of chamomile and poppy heads as hot as it can be comfortably borne; and immediately after being well dried, to drop into the internal canthi of the eyes the following Tinc-

tura opii mitis. Take of purified opium, of English saffron, each two drachms; white French Brandy, an ounce; distilled water seven ounces. Let them stand in a close vessel for six days, then filter for use. Apply frequently during the day a collyrium of cerussa acetata, made warm; wear a shade over the forehead, and adopt the antiphlogistic regimen. In a few days repeat the calomel and purging medicine. When the leeches cannot be applied, smart purging must be continued for some days. After the tenderness of the sight has been removed by these means, tonic collyria, purgative pills, and Peruvian bark are recommended to complete the cure. The collyria, preferred by the author, are solutions of white vitriol in water, either alone, or with a very small quantity of oil of vitriol and brandy: they should all be transparent, and applied cold.

Whenever the eye has been universally dry, accompanied with a slight degree of heaviness about the orbit, a snuff of powdered fox-glove has afforded considerable relief; tending most powerfully to unload the contiguous vessels of the eye. In a note, we are told, "In the lists of Errhines, I do not recollect to have seen the foxglove included; a herb which, I believe, is scarcely known to possess the peculiar property of that class of remedies. From experience, however, I find that when used in its active state, it excites a very copious excretion of thin mucus from the membrane lining the nostrils, without producing any narcotic effects.

"This fact was first communicated to me by an eminent physician in the country, who became accidentally acquainted with it, by observing an old woman using it as a substitute for common snuff."

In addition to the tonic remedies enumerated, we would mention one which we have found to be very powerful in cases of morbid sensibility of the eyes, from ophthalmia, and which we believe is seldom used. We allude to the air of the country, which has in several instances been found beneficial when all other remedies had proved ineffectual.

MEDICAL INTELLIGENCE.

Extract of a letter from Dr. David Hossack, Professor of the Theory and Practice of Medicine in the University of New-York, to one of the editors of the Eclectic Repertory, dated New-York, March 18, 1811.

"I will conclude this letter by stating to you, that on the first of February last I delivered a lady in this City, Mrs. C—, about twenty two years of age, who had not menstruated for near two years before her pregnancy. The suppression, which took place about eighteen months before her marriage, was the effect of cold, occasioned by getting her feet wet at the time her menses were flowing; but prior to that suppression, her catamenia had been regular, both as to quantity and the periods of their return. About six months before her marriage, she had a very small discharge of a blackish matter from the uterus; but so inconsiderable, that she states it to have been *a mere show*. Since her marriage, which took place in October 1809, she has not had the least return of her catamenia, either prior to or since her pregnancy. Her labour was in all respects natural, without any extraordinary discharge after the separation of the placenta, which also came away spontaneously. The lochial discharge continued the usual period; and she makes an excellent nurse, having plenty of milk for her child. This fact, of her proving pregnant after two years' absence of the catamenia, being very unusual, and contrary to the observations of the most respectable writers, I thought might prove interesting to you as a teacher of midwifery."

Vaccination in Glasgow.

The following statement contains perhaps one of the most decisive proofs of the utility of vaccination which has been submitted to the public. The first column contains the year. The second the number who have died of the small-pox in the city. The third the whole number of deaths in the city. The fourth the number of deaths in the city and suburbs.

First.	Second.	Third.	Fourth.
1792	403	1508	1912
1793	134	1356	2190
1794	278	1365	2445
1795	132	987	1700
1796	265	1327	2297
1797	134	961	1813
1798	231	1125	2064
1799	179	1025	2181
1800	224	1279	2499
1801	159	985	2096
1802	107	825	1928
1803	91	1158	2438
1804	123	1011	2224
1805	21	968	2389
1806	15	939	2280
1807	48	1002	2463
1808	14	1446	3265
1809	54	——	2368
1810	23	1121	2367

There are perhaps few towns of the same magnitude where the beneficial effects of vaccination have been more distinctly experienced. None of those jarring opinions which have disgraced other parts of the kingdom, are known in Glasgow. The profession universally recommend the practice, and the people almost as universally receive it. The few deaths by small-pox, which have occurred, within these last six years, have been exclusively among recent incomers, and the poorest and most wretched of the Highlanders and Irish. It appears from the records of the Vaccine Institution, that, previous to January, 1811, upwards of *fourteen thousand five hundred* have been inoculated *gratis.—Edin. Jour.*

Report of the Surgeons of the Vaccine Institution of Edinburgh, 1810.

In reporting to the managers of the Vaccine Institution the state of vaccination for 1810, the surgeons have the satisfaction

to mention, that nothing has occurred which can in any degree diminish their belief of the perfect efficacy of the cow-pox, as a preventive of the small-pox.

Since last report, 583 have been vaccinated, making in all 11,108, from the commencement of the practice at the Institution.

The surgeons have, since last report, inoculated with the small-pox a great many children, who have been vaccinated eight, nine, and ten years ago—all of whom have been found to resist the infection.

In no instance have they seen any bad effects which could be attributed to vaccination; and, upon the whole, the experience of another year serves to confirm them in their former opinion, that the practice of vaccination is deserving of the highest degree of confidence from the public.

Signed by WM. FARQUHARSON.
JAMES BRYCE.
ALEXR. GILLISPIE.
JOHN ABERCROMBIE.

Edinburgh, Jan. 21, 1811.

The resources with which Nature is provided for distributing the vital fluid through the bodies of animals, when the principal trunks of arteries are destroyed, has been remarkably exemplified in experiments lately made by Mr. Ashley Cooper. That gentleman tied the *aorta descendens* of dogs, very near to the heart, in a way to stop the current of blood passing, by that vessel, to all the lower parts of the frame. The animals seemed to sustain no great inconvenience by this: the wounds soon healed, the health was not impaired, the secretions proceeded as usual, and the creatures remained active and lively. When they were killed after some weeks, or months, for the purpose of ascertaining the changes that had happened, from the destruction of a part presumed to be so essential to life, the aorta was found obliterated where the ligature had been fixed, and the blood had been transmitted by anastomosing branches.

A marked instance of the appearance of Small-pox twice in the same person has occurred in the case of the Rev. Mr. Rowley, son of lady Rowley. About forty years ago Mr. Rowley, then a child, was inoculated for the Small-pox, by Mr. Adair, Surgeon general, and had a considerable eruption. On the 5th of June last, he was seized with fever, and an eruption appeared on the third day: there were two hundred pustules on the face, and the distemper has proved a severe case of distinct Small-pox.

Another instance of Small-pox after inoculation has happened to Miss S. Booth, of Covent Garden Theatre. At five years of age this young lady was inoculated for Small-pox. The progress of the arm was regular, she had considerable fever, and the whole of the appearances were of a nature to afford, it was believed, a perfect security from any future attack of the disease. On the 20th of June, she was seized with febrile symptoms, which proved the precursor of Small-pox: on Sunday, the 3d day from the attack, pustules appeared on the forehead and scalp. The eruption spread to other parts of the frame, accompanied with sore throat. This eruption passed through the usual forms and stages of the disease, and constituted an undoubted case of *Variola.*

A paper by Mr. Home has been read before the Royal Society, on the nonconducting powers of the thoracic duct and the spleen from the stomach to the bladder. Mr. H. related a number of experiments made with ligatures, or by removing the spleen, and keeping a ligature on the thoracic duct, in which state, rhubarb was taken into the stomach of the animal, and was detected in the urine; whence he inferred, contrary to some opinions he formerly published, that those two organs are not necessary to the secretion of urine.

Mr. Brodie has read before the same society a paper on the effects of vegetable poisons on animals. The author has pursued his researches for a considerable time, and detailed to the society the result of his experiments on rabbits, cats, and dogs, with alcohol, oil of bitter almonds, extract of aconite, tobacco, &c.

These vegetable substances thrown into the stomachs of dogs, cats, rabbits, &c. instantly killed them by acting on the nervous system, and producing a compression of the brain: thrown into the rectum, the same effects were produced. The pulsation and heat of the heart, after administering these poisons, were maintained for a considerable time by means of artificial respiration, except with tobacco, which instantly destroyed the powers of the heart, and arrested the pulsations. One drop of the empyreumatic oil of tobacco let fall on the tongue of a cat killed her, but did not destroy the pulsation so instantaneously. Mr. Brodie made a great number of experiments with the vegetable poison used by the American Indians to poison their arrows, and with nearly similar results.

We learn, that Dr. Rousseau of this city has ascertained by a series of original experiments, which were afterwards repeated in conjunction with Dr. Samuel Smith, that the effects of the effluvia of a variety of deleterious substances are not owing to absorption from the lungs into the circulation, but to an operation exclusively on the olfactory nerves. We are promised by these ingenious experimentalists, a communication on this subject for the next number of our journal.

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Communications relative to the Datura Stramonium, or Thorn-Apple; as a Relief or Cure of Asthma. Addressed to the Editor of the Monthly Magazine; many of them never before published.

N. B. This pamphlet contains not only full and clear instructions for the growth, preparation, and administration of Stramonium, but also satisfactory testimonials of the efficacy of the genuine, unadulterated, and unsophisticated herb, from professional men and others. 3*s*. 6*d*.

Practical Observations on Insanity. To which is affixed, an Account of some Diseases incident to the Insane. By Bryan Crowther, Member of the Royal College of Surgeons, London; and Surgeon to Bridewell and Bethlem Hospitals.

A familiar Dissertation on the Causes and Treatment of the Diseases of the Teeth, with explicit instructions for their management so as to render them white and firm in their socket, and to preserve them from caries, tooth-ach, &c. By J. P. Hertz, Surgeon Dentist.

An Account of the Ravages committed in Ceylon by Small-Pox, previously to the Introduction of Vaccination: with a Statement of the Circumstances attending the Introduction, Progress, and Success of Vaccine Inoculation in that Island. By Thomas Christie, M. D. Member of the Royal College of Physicians, London, and of the Royal Medical Society, Edinburgh; late Medical Superintendent, Geneva, in Ceylon.

Surgery.

The Modern Surgeon; or, Plain and Rational Rules for the Direction of Practice; founded on the Observations and Experience of the most distinguished Practitioners. 8*s*. 6*d*.

Practical Observations on the Diseases of the Inner Corner of the Eye, comprising the Epiphora, the Sacculus Lachrymalis, and the Fistula Lachrymalis. With a new Arrangement and Method of Cure. Also Remarks on Mr. Ware's and Professor Scarpa's Methods of treating these Disorders. By Joseph Read, M. D. 5*s*.

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The Archives of Useful Knowledge. By James Mease, M. D. No. 1 of vol. 2.

This is a journal of extensive usefulness. It is eminently adapted to diffuse that sort of information on Agriculture and the Useful Arts which is, at present, sought after with such avidity in this country. We wish success to an undertaking so laudable, and, in every respect, so well conducted.

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An Essay on Diseases incidental to Europeans, in Hot Climates, and the method of preventing their fatal consequences. By James Lind, M. D. F. R. S. Ed. &c. To which is added an Appendix, concerning Intermittent Fevers, and a simple and easy way to render Sea Water fresh, and to prevent a scarcity of provisions in long Voyages at Sea. William Duane, Philadelphia.

Observations on the Changes of the Air, and the concomitant Epidemical Diseases of the Island of Barbadoes; to which is added, a Treatise on the Putrid Bilious Fever, commonly called the Yellow Fever; and such other Diseases as are indigenous or endemial, in the West India Islands, or in the Torrid Zone. By William Hillary, M. D. With Notes, by Benjamin Rush, M. D. Professor of the Institutes and Practice of Medicine in the University of Pennsylvania. B. and T. Kite, Philadelphia.

A Description of the Arteries of the Human Body, reduced to Tables. By Adolphus Murray, M. D. R. and O. Professor of Anatomy and Surgery at Upsal. Translated from the original by Archibald Scott. Thomas Dobson, Philad.

In Press,

A Treatise on the Process employed by Nature in suppressing the Hemorrhage from divided and punctured Arteries; and on the use of the Ligature; concluding with Observations on secondary Hemorrhage. The whole deduced from an extensive series of Experiments, and illustrated by fifteen plates. By L. F. D. Jones, M. D. member of the Royal College of Surgeons, London. T. Dobson.

THE

ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

Vol. II. JANUARY, 1812. No. VI.

SELECTED PAPERS.

On the Staphyloma, Hydrophthalmia, and Carcinoma of the Eye.

By JAMES WARE, Esq. F.R.S., and Vice-President of the Medical Society.

[From the Transactions of the Medical Society of London. Vol. I.]

ALTHOUGH it be too often the melancholy province of medical men to witness disorders which cannot be removed, and in the treatment of which the utmost exertions of their art can only produce a mitigation of the symptoms, surgery nevertheless, besides affording in this way considerable benefit to mankind, is often highly serviceable in various diseases, by preventing the occurrence of greater evils than those which have already taken place. This observation is strikingly exemplified in those disorders of the eye to which the attention of the Society is now requested; for though all of them have irrecoverably destroyed vision, yet the staphyloma and hydrophthalmia indispensably require an operation that shall cause the eye to sink in the orbit, in order to obviate constant pain and uneasiness; and the carcinoma of the eye is only capable of receiving a check, in its tendency to destroy life, by the complete extirpation of that organ.

The term staphyloma is sometimes used to designate the protrusion of a part of the iris through a wound or ulcer of the

cornea. This is perhaps its most correct meaning, the Greek word, from which it is derived, implying similitude to a raisin or dried grape. But various authors have also used the term to denote a projecting opake cornea; and in this sense I propose more particularly to employ it at this time. When the projection is very considerable, the disorder is sometimes also called proptosis; and in those cases where the projection is not confined to the cornea, but occupies also a portion of the sclerotica, as sometimes happens, this latter appellation is peculiarly appropriate.

It has been disputed by authors whether the projection of the opake cornea, in the staphyloma, is occasioned by a thickening of this tunic, or by a morbid accumulation of aqueous humor behind it. I believe, in general, both these circumstances combine to produce the disorder; the cornea becoming not only opake, but both softer and thicker than in its natural texture; and, in consequence of this, the aqueous humor behind the cornea pushes it forward, and thus enlarges the anterior chamber of this humor. I have sometimes seen the whole cornea sloughed off during an acute purulent ophthalmy, and a white opake substance gradually effused from the ulcerated surface, sufficient to form a complete cover to the iris; after which this opake body has gradually projected in a conical shape, until at length it has become so prominent as to hinder the eyelids from closing over it. I have at other times seen the projecting cornea partly opake, and partly transparent; the pupil being distinctly visible through the transparent part, but the power of vision wholly destroyed. Sometimes the circumference of the opake cornea projects, its central part appearing depressed, and resembling the bottom of a plate or dish; and sometimes, near to the center of the opacity, in the case last mentioned, there is an irregular black appearance, which a cursory observer might mistake for a pupil. No part of this aperture, however, is perceptible on a careful inspection, and the eye of course is deprived of all useful vision.*

* The cornea not unfrequently projects without losing its transparency, assuming a conical shape instead of that which is spherical; in consequence of which change the eye at first becomes myopic; but when the projection is more advanced, it causes so unequal a refraction in the rays of light as to

So long as the projection of the opake cornea can be covered by the eyelids without painfully stretching them, if it be not accompanied with an irregularity in the surface of the cornea, and the sight of the other eye continue perfect, the only inconvenience the projection occasions is produced by the unseemly appearance it presents to observers. This may in some degree be prevented by wearing a pair of spectacles containing plain window glass in the ring opposite the sound eye, and glass that is ground in a slight degree opake, or even similar plain window glass, in the ring opposite the affected eye. In some instances, however, a consciousness of the appearance produced by a projecting opake cornea has occasioned so much distress of mind, that I have been requested to sink the eye, solely for the purpose of getting rid of the deformity. I wish I could say that milder means have been found sufficient to accomplish the object. Various applications have been proposed for this purpose at different times by different authors. By some, strong caustics have been recommended for the express purpose of producing an excoriation, and even an ulceration, on the surface of the projecting substance. Both *Janin and †Richter have said they not only removed the projection of an opake cornea, but even reproduced its transparency, by the application of the butter of

destroy correct vision. In cases of this description I have repeatedly discharged the aqueous humor, and endeavoured afterwards, by moderate pressure, to prevent the return of the projection; but on the reproduction of the aqueous humor the conical projection has always reappeared. If only one eye be thus affected, the sight of the other remaining perfect, all the purposes of vision will be obtained from this alone; but, if the cornea of both eyes be conical, much advantage may be obtained from wearing spectacles, the rings of which are filled with an opaque substance that has a small hole in its center, not more than the tenth or twelfth part of an inch in diameter, the smallness of which aperture, by lessening the pencils of the rays of light, will prevent the confusion that must otherwise be occasioned by their unequal refraction. Persons who have a projecting cornea should be particularly careful to avoid blows on the eye; since the projection is usually accompanied with a preternatural thinness of this tunic, which renders it easily ruptured: when this happens, the iris is liable to be involved in the wound, and the sight to be more or less injured by the derangement that takes place in the figure and size of the pupil.

* Janin sur l'œil, sect. 8, page 389 et sequent.

† Richter, fasciculus 2, page 105 et sequent.

antimony. Janin has recommended this application, for the purpose also of removing that other spceies of the staphyloma, in which there is a protrusion of part of the iris through an ulcer of the cornea. But I beg leave to observe that caustic applications of every kind should be used with great caution in all diseases of the eye. I have known them occasion violent and long continued inflammations; and, so far from reproducing vision, they have very rarely reduced the prominence of the staphyloma so as to preclude the need of other means to take away the deformity. Scarpa, in his chapter on the staphyloma, expresses himself in a similar way; and has adduced several cases of this disorder in children, in whom an ulceration on the surface of the cornea was kept up by escharotic applications several weeks, and yet no diminution was obtained by it, either in the projection or opacity. If such be the result of the experiment on the eyes of children, it certainly is less likely to succeed on those of adults. The other mode which has been proposed by authors, viz. that of compressing the tumor, and thus restraining it from interfering with the motion of the eyelids, is so difficult to be accomplished, with the necessary accuracy, that I remember only one case in which it afforded any advantage. In this instance a poor man who had a staphyloma of one eye many years, and could not be prevailed on to submit to have the eye sunk, was kept easy by wearing a bandage round his head, not unlike to the spring truss that is used for an inguinal hernia. The bolster of the instrument made a pressure on the outside of the eyelids, which kept them constantly closed, and hindered the eye from moving. In consequence of this, the projection gave no pain; and, by the aid of the other eye, the patient was enabled to work at a common handicraft business without inconvenience.

The more direct way of affording relief in the staphyloma is by removing the whole of the projecting substance; in consequence of which the humors of the eye are discharged, and the posterior part of its tunics collapse, so as to form a kind of button at the bottom of the orbit. On this button, when the wound is healed, an artificial enamelled eye is capable of resting; by which the uniform appearance of the face may be restored. Authors are not agreed on the best mode of perform-

ing the operation. Heister, St. Yves, and others, have proposed to pass a double ligature through the middle of the tumor, and then to separate the threads, and tie the tumor on each side, so that the compression made by the ligature may cause it to mortify and slough off. But this is so painful, and so indirect a mode of accomplishing the object, that I believe it has not been practised for many years. Scarpa, in more modern times, has recommended to us to remove a small portion only of the projecting cornea (agreeable to a mode first proposed by Celsus in his book de medicina, lib. vii, cap. 7.*), and to force out the crystalline and vitreous humors through the opening; after which, he says, the wound will close, and the tunics of the eye collapse to a small size, without occasioning any considerable degree either of pain or inflammation. This mode of performing the operation appears to me, however, to be liable to considerable objections. If the opening in the cornea be not larger than the size of the crystalline humor (which not unfrequently, in cases of the staphyloma, is without disease), this humor, in passing through the aperture, is very liable to bruise the iris, and to bring on pain and inflammation, that are both violent and tedious; and if, on the contrary, the opening be so large as to allow the crystalline and vitreous humors to be discharged, without doing violence to the iris, though the pain and inflammation, consequent on the operation, may not be considerable, yet the place of the evacuated humors will be supplied by a watery humor, which will speedily distend the tunics of the eye to their former size, will do away the possibility of inserting an artificial eye, and will hazard the return of all the old symptoms. Scarpa, aware of these circumstances, mentions expressly, that he has been obliged to irritate the wound three or four different times, after the operation, in order to bring on a sufficient degree of inflammation to cause the eye to collapse. Influenced by these considera-

* The words of Celsus are, "in summa parte ejus ad lenticulæ magnitudinem exscindere." Scarpa proposes to make an opening "two, three, or four lines in diameter, according to the size of the "staphyloma;" but the largest of these dimensions being only one third of an inch, is barely sufficient to allow the crystalline to come through it, without forcibly compressing the iris.

tions, I have never performed the operation according to this method; and having uniformly succeeded in a considerable number of cases, during a practice of more than thirty years, by performing it in the following manner, I trust that I am justified in recommending my mode of operating to the attention of this Society.

The operator will find it more convenient to stand behind the patient than before him; and the patient should be placed on a chair sufficiently low to allow the operator to carry his hand with ease over the patient's head. A large crooked needle, armed with a strong thread, should then be passed through the opake projecting cornea, and, after separating the needle from the thread, a knot should be tied in the latter, at a small distance from the eye, in order to hinder the thread from slipping. The operator having thus obtained by means of the thread a secure hold of the eye, a knife similar to that which is used to divide the cornea in extracting the cataract, or, if this be not at hand, a long sharp-pointed lancet, should be pushed through the sclerotic coat, about a quarter of an inch from its connexion with the cornea, and be carried quickly but accurately round the cornea, as nearly parallel to it as can be accomplished. Sometimes, as soon as a puncture is made through the sclerotica, so large a portion of the vitreous humor escapes, as to cause the cornea to become flaccid; in consequence of which the operator may find it difficult to complete the incision round this tunic with either the lancet or the knife: and in this case a curved blunt-pointed scissars will be found useful to finish the operation. The only objection to the use of the scissars is drawn from the additional pain which it is supposed to give; but the duration of the operation is so short, that the difference between the pain produced by the instruments is scarcely worthy to be named. The hemorrhage that succeeds is seldom considerable; and the less the eye is examined afterwards, the less danger will there be of pain and inflammation. A compress wet with a saturnine lotion should be applied over the eye, and it should be moistened with this liquor, without being removed, as often as it becomes dry; but no lint or any other application should be put within the lids, since this has been known to give great pain, and in one in-

stance to occasion alarming symptoms. An anodyne should be given after the operation, of greater or less strength according to the age of the patient; but it is seldom necessary to repeat this medicine, since the patient has usually more sound and quiet sleep after the operation than he had for a long time previous to its performance. At the end of about a fortnight that part of the sclerotica which remained in the orbit will be found to have collapsed, and sometimes a small fungous substance will then protrude through the wound. This in the course of time would subside of itself, but, as the delay may be irksome, the fungus may be easily removed, and with very little pain, by snipping it off with a pair of sharp scissars. The fungus is usually smaller in its neck where it joins the sclerotica than in its top; in consequence of which its removal is effected with very little difficulty; and though it sometimes reappears, it may be snipped off, again and again, until at length the wound will completely close, the inflammation cease, and the orbit become fit to receive an artificial eye. This, however, ought not to be introduced until the inflammation be perfectly removed; and when such an eye is used, it is advisable to withdraw it every night and replace it in the morning, which may be effected with ease by the patient himself, after a short experience. In the choice of the artificial eye, it is not only important that the color of the iris resemble accurately that of the sound eye, but the size of the eye should be well adapted to that of the orbit, and the dimensions of the cornea be rather smaller than that of the natural eye. If these rules be not regarded, the artificial eye will give an unsightly stare to the countenance; it will not move, as it ought to do, in unison with the sound eye; and it will be liable to occasion both pain and inflammation. It is of consequence also to know that an artificial eye is apt to irritate after it has been used about a year and a half or two years, and must then be either disused entirely, or its place be supplied by a new one: and it may not be improper to remark, that when an eye has been sunk, if an artificial eye be not introduced, the appearance of the countenance may be much improved by wearing a pair of spectacles with either plain window glass in the circles, or glass that is tinged in a slight manner with a green or blue color. The

reflection from the glass in the spectacle frame will prevent the deficiency from being noticed, or will only give rise to the supposition of the eye being weak.

I next proceed to consider the disorder called Hydrophthalmia. By this term authors do not in general mean an accumulation merely of the aqueous humor, but so great an enlargement of the whole eye, produced by an increase of the vitreous humor as well as the aqueous, as to cause the eye to occupy an undue portion of the orbit, and to occasion difficulty and pain when the eyelids are closed over it. Thus defined, it may perhaps with more propriety be denominated Exophthalmia than Hydrophthalmia.* In describing this disorder a greater discrimination is required than seemed necessary in the former part of this paper. In the staphyloma, for instance, the opake projecting cornea designates the nature of the disorder in so plain a manner, that it seems impossible to make a mistake with regard to its nature. But in the hydrophthalmia, which implies an universal enlargement of the eye, some examination is requisite in order to ascertain what occasions the enlargement; whether there be an equal enlargement of all the different parts of the eye; a morbid enlargement of one particular part only; the formation of an adventitious body within the eye; or a projection of the eye in consequence of a substance formed behind it.

Infants are sometimes born with eyes remarkably large and prominent. But if they do not give pain by their pressure, nor interfere with the free motion of the eyelids, and if at the same

* Scarpa is of opinion that an accumulation of water between the choroid coat and retina is a common cause of the hydrophthalmia, and he minutely describes a case of this kind which occurred in a child three years and a half old, in which the eye was a third larger than its natural size, the cornea partaking of the increase, in the same proportion as the sclerotica. I have several times observed, on dissecting the eye after death, that there has been an effused fluid between the choroid coat and retina, the vitreous humor being wholly absorbed, and the retina collapsed into a cylindrical, or rather a conical, chord-like substance, its apex arising from the optic nerve, and its basis surrounding the crystalline humor; but, though this effusion had produced a fixed dilatation of the pupil, an opacity of the crystalline, and sometimes a violent deep seated pain in the eye, I have never known it to occasion an enlargement of this organ.

time the cornea be transparent and the sight perfect, the mere circumstance of their prominence does not call for any particular attention. Sometimes, however, the eyes of infants, at the time of their birth, are not only remarkably prominent, but the cornea of one or both is universally opake, without any accompanying inflammation in the conjunctiva, or any morbid discharge from the eyes. Of this I have seen several instances, three of which happened in one family. These were more directly under the care of Mr. Farrer, a surgeon, resident at that time at Deptford. He has described them with accuracy in the second volume of Medical Communications, page 463, published in London in 1790. The opacity gradually diminished; and in less than a year, in two of them, it was quite removed. In the third the cornea did not resume its transparency until the end of the second year. The amendment in these instances cannot be attributed to any particular remedies, since none were used; but it was owing to the *vis naturæ medicatrix*, which in infants, in this disorder, as it also is in many others, is often effectual to restore a healthy state. Mr. Farrer does not mention any particular prominence in the eyes of these children; but, having seen two of them shortly after the time when Mr. Farrer drew up the account of the cases, I find, by a minute I then made, that the cornea appeared to me remarkably prominent; and that, though the children had recovered a distinct vision, they were all shortsighted.—Another case of a similar kind came under my notice about three years ago, in the new born infant of a respectable farmer in Essex. Both corneæ were completely opake, and both were large and prominent. In this instance, as in those last mentioned, no applications were used with sufficient steadiness to allow me to attribute any considerable degree of efficacy to them; notwithstanding which, when, about four months ago, the child was again brought to me, I had the satisfaction to see the left cornea sufficiently clear to allow the perception of all large objects; the opacity of the right cornea being also diminished round its outer edge, though the greatest part of the pupil was still obscured. I was consulted in a fifth case of the same kind about a year ago. It occurred in the infant of a gentleman in Portman Square. Here, as in the other in-

stances, the corneæ of both eyes, at the time of birth, were large and prominent, and they were at the same time completely opake; the child, in other respects, being healthy, and suffering no pain from the state of the eyes. Sanctioned by the successful issue of the preceding cases, no particular remedies were employed; and at the time of my writing this paragraph, which is just a year from the birth of the child, the cornea of one eye is not only perfectly transparent for a considerable space round its circumference, but the pupil can be seen through the diminished opacity that remains in its center; and though the cornea of the other eye has improved less in its appearance, the transparency of this also is evidently increased, and the iris is visible through it, for the space of a line at least round its rim.

In all these instances, the enlargement of the eye was not sufficient to be of serious consequence independent of the opacity of the cornea; and, when this opacity was dissipated, the power of vision was restored. But when, on the contrary, the enlargement is not confined to the cornea, but extends to the sclerotica, and is so considerable that the eyelids cannot be closed without difficulty, the patient being not only blind, but unable to sleep without the aid of opiates; the prospect of restoring sight is wholly lost, and the only question is, in what way ease may be obtained, and deformity obviated. It does not appear possible to do more than this; nor can even this be accomplished by any other mode than that of diminishing the size of the eye: and the best manner of doing it I believe to be by means of the operation, which has been recommended above in cases of the staphyloma.

Before an operation of so much importance be performed, it is, however, essentially requisite to ascertain that the disease consists solely in an enlargement of the different parts of the eye; and that it is not produced by the formation of purulent matter within the eye; by a morbid alteration in the structure of either its coats or humors; nor by the undue accumulation of adeps, or of any other substance, behind this organ.

When purulent matter is accumulated within the eye, the inflammation and pain, which both precede and accompany the enlargement, seem fully sufficient to distinguish the pecu-

liar nature of the disorder; and they at the same time point out the necessity of procuring an adequate aperture in the tunics of the eye, through which the matter may be discharged. In a case of this kind, which I was desired to see at a small distance from London, in which a young lady, nine years of age, had suffered agonizing pain several days, the sight of the eye having been lost many years, and the cornea being both opake and prominent, an aperture had taken place spontaneously on the side of the eye next the temple, just in that part where the cornea is joined to the sclerotica, and through it a small portion of matter had escaped; but the tension of the eye continued, and the wound was only large enough to admit the blunt end of a probe. The propriety of enlarging the aperture naturally suggested itself; and as the eye had not been useful for a long time as an organ of vision, a small blunt-pointed bistoury was immediately introduced through the wound, to the depth of at least a quarter of an inch, and the incision was carried three quarters of an inch in a direction towards the temple, dividing at the same time the sclerotica choroides and retina, and making a large opening into the body of the vitreous humor. No part of this humor, however, nor any sort of fluid, issued through the wound at the time of the operation. The eyelids were immediately closed, without any pressure being made on the eye, and directions were given to apply an anodyne fomentation, in the same way in which it had been frequently before used. An anodyne draught was intended to be given, but within half an hour the patient fell into a sound sleep, which lasted several hours. She awoke much refreshed and perfectly easy. The wound discharged more or less of matter for a fortnight; the pain did not return; and the eye gradually diminished, so that in a short time it did not appear to be more than one half of its natural size.

Purulent matter is sometimes also formed behind the eye in the adipose substance that supports this organ in the orbit. If the suppuration be quick in its progress, and be not situated deep, the fluctuation of the matter may be easily felt, and the propriety of discharging it be determined at once; but if, as I have occasionally found, the suppuration be slow, and the matter lie considerably below the surface, the eye will be protru-

is perceived in the eye, the sight is impaired, and, in a short time, it is wholly lost. At its commencement it bears a slight resemblance to a cataract; but an attentive person will at once discover the difference between the two disorders; the opacity in the cataract lying close behind the pupil, whilst in the fungus hæmatodes it is situated deep in the posterior part of the eye. In the cataract, the pupil retains the power of dilating and contracting in different degrees of light; but in the fungus hæmatodes the pupil never varies its size, and is usually dilated. When the disorder has so much advanced as to destroy the figure of the eye, and to make it protrude beyond the rim of the orbit, it is more difficult to distinguish it from what has usually been called a carcinoma of this organ. There is still greater difficulty, when, after extracting an eye that contains a fungus hæmatodes, a fresh tumor arises from the bottom of the orbit, which fills this cavity, and continues increasing, until it becomes, as has sometimes happened, as large as the whole head. This difficulty of distinguishing between the carcinoma of the eye and the fungus hæmatodes is however, the less to be regretted, since the proper treatment of both disorders seems nearly alike; the only known mode of checking the progress, in both, appearing to be the complete extirpation of every part that is diseased. Before recourse be had to the operation, it is necessary to ascertain, as far as possible, that every such part is capable of being removed; since, in both disorders, if the smallest portion that has been contaminated remain, whether it join the organ that is extirpated, or be at a distance from it, the diseased part will infallibly increase, and all the old symptoms be reproduced. The fungus hæmatodes is not always confined to one eye, nor even to both, but sometimes occupies a large portion of the orbit exterior to the tunics of the eye. It is also accompanied not unfrequently with abscesses and tumors in different parts of the head; sometimes between the pericranium and cranium; and at other times between the pericranium and dura mater. These abscesses are not confined to the fore part of the head, having sometimes been found both on the outside and inside of the os occipitis. Distinct portions of matter, and sometimes hard tumors, have also been formed in the dura mater, and even in the substance of the cerebrum; and sometimes under the an-

terior lobes of the cerebrum, making a compression on the thalami nervorum opticorum. A disease of this kind is by no means new. It has occasionally come under my notice ever since I was a boy, and it has been described by many of our ancestors under the common name of carcinoma or cancer. It may be more correct, however, to distinguish it by the term fungus hæmatodes, or medullary sarcoma, though it does not appear to me to be always easy to ascertain the difference between the two disorders. It has been said that carcinomatous affections are always preceded by a hard circumscribed tumor, and that, after an ulceration has been produced, if it be followed by a fungous excrescence, this is of a cauliflower figure, and a hard firm texture; but such cannot be admitted to be the universal progress of these affections, nor is it unlike to that which the fungus hæmatodes sometimes assumes. It may be said with greater correctness that the carcinoma of the eye is a disease to which persons are most subject in the middle or latter part of their lives, whereas the fungus hæmatodes appears in early life, and most commonly in infancy.

The following is the progress of a disorder which I have also repeatedly seen in persons advanced in life, but do not remember in any who were young. By some it may be called fungus hæmatodes, and by others carcinoma, but I shall content myself with describing it. The sight is lost before any change takes place in the appearance of the eye: after this the pupil becomes dilated without any visible opacity in the crystalline humor. This description designates a gutta serena; but the disorder does not stop here. After a little time the crystalline humor becomes opake; and soon afterwards shooting pains are experienced, which dart suddenly through the eye in different directions, rarely continuing long at one time. At this period, if the sclerotica be carefully examined, a bluish, or rather a dusky leaden colored, spot, of greater or smaller extent, will be discovered in it, on one side of the cornea, and sometimes on both. These bluish or leaden colored spots gradually spread; the eye enlarges either partially or generally; and in a short time it pushes forwards the eyelids, and fills the whole of the orbit. In some instances the bluish enlargements appear as if they were affections of the outer surface of the

sclerotica, and only covered by the tunica conjunctiva. In others they are evidently produced by a distention of the whole substance of the sclerotica, which is pushed out and thinned, where the projection appears by the accumulation of a morbid substance within the eye. A few of the bloodvessels of the conjunctiva are usually enlarged, and have a purplish red appearance, very different from that which is produced by a common inflammation. On examining the internal state of these tumors, after their extirpation, the whole of the eye has been found full of the leaden colored substance I have described; divided, in an irregular manner, by membranous laminæ into separate cells, the contents of which have varied much, even in the same eye, in their degrees of consistence. They are usually firm and solid, but sometimes contain pus in separate cysts, and sometimes also osseous particles that differ much in their shape and size. These tumors are in general produced by an irregular enlargement of the whole eye, involving both its coats and humors; but sometimes the humors are very little altered, the disease seeming to originate in an affection of the tunica sclerotica, which spreads outwards rather than inwards. Sometimes the tumor is confined to one side of the eye, its other side being unaffected. At other times it occupies both sides; and, occasionally, there have been three tumors annexed to the eye, one on each side and one above, all as large as the eye; this organ being unaltered in size, though deprived of sight.

The progress which the disorder makes is very various. Sometimes a prominence of a leaden color has continued in the substance of the sclerotica, on one side of the cornea, many years, without giving pain or occasioning any sort of trouble; and, on the contrary, it has at other times increased rapidly, and the enlarged organ in a few months has completely filled the orbit.* It does not appear that medicines or

* Since these papers were put together I have extirpated an enlarged eye from a gentleman, thirty years of age, who had lost the sight of it many years; but it occasioned no pain or inconvenience until about six months ago, when it began to enlarge, and an increase in its size had afterwards been perceived almost every week. The enlargement of the eye was universal; the bloodvessels had a purplish red appearance; there were three bluish

applications have the power of checking or controlling this malignant disorder; and whenever its nature can be clearly ascertained, the only question is, whether it be possible to extirpate completely every part that partakes of the poison. Although it be a melancholy truth that the operation has too often failed, this does not lead to the conclusion that its performance is always improper, since it certainly has not unfrequently succeeded; and I have the satisfaction to say, though I have sometimes failed, I have several times performed it with complete success.

With regard to the mode of performing the operation, I would advise it to be done in the following manner.

The patient should be seated in a clear light, on a chair of a suitable height to bring his eye on a level with the breast of the operator; and the operator should either sit or stand before him, as is most easy to himself. The patient's head should rest against the breast of an assistant, whose left hand should support the upper eyelid by means of a double blunt-pointed hook, the points of which are seven eighths of an inch distant from each other, and his right hand should be at liberty to do any thing that may be desired by the operator. The hands of the patient should be held by two assistants that sit, one on each side, and an assistant should be ready to give the operator instruments, sponges, &c. A crooked needle armed with a strong thread, and well waxed, should then be passed through the whole of the cornea; after which, the needle being cut off, a knot should be tied in the thread, at the distance of about an inch from the eye, to hinder it from slipping. This thread is more useful in cases where the eye is so much enlarged as

spots on the sclerotica, one of which was as large as a sixpence; and the pressure of the eye against the eyelid kept up a constant uneasiness. In a consultation with Mr. Cline, it was judged advisable, as the increase of the tumor was rapid, to recommend the extirpation of it without delay; and the patient giving his consent, I performed the operation, in the presence of Mr. Cline, a few days after the consultation. Nothing unusual occurred at the time; and on examining the tumor afterwards, the humors of the eye were found to be no otherwise affected than by their enlargement, the blue appearances being occasioned solely by an affection of the sclerotica. No accident happened after the operation, and, in less than a month, the wound was healed, and the patient returned, perfectly well, to his home in Kent.

nearly to fill the orbit, than when it is smaller; the finger alone, in the former case, being insufficient to incline the tumor from one side to the other, so as to make the room that is required for the proper use of the knife. If the tumor be considerable, the upper and lower eyelids should next be separated, by dividing with the knife the integuments which unite them on the side next the temple. This will give much additional room for the introduction of the knife to dissect the diseased organ from its attachments. The conjunctiva should then be divided round the whole globe of the eye; and afterwards the knife be carried downwards, on that side where it passes with the greatest ease. It is not possible to give precise directions, as to the mode in which the dissection should be conducted; but great care should be taken to avoid wounding the tumor until the point of the instrument has reached the bottom of the orbit. If it be possible, the operator should introduce his finger with the knife so as to feel the optic nerve, which, together with the muscles of the eye, should be divided as close to the foramen opticum as the instrument can be carried. In general the common straight scalpel may be so directed as to perform this part of the operation with accuracy; but if the tumor completely fill the orbit, it may be useful, in this part of the operation, to substitute for the straight scalpel one that is a little curved. As soon as the optic nerve and muscles of the eye have been divided, the tumor becomes loose, and may be easily drawn out of the orbit, either by the fingers, or by the ligature that was passed through the cornea at the beginning of the operation. The tumor, when removed, should be carefully examined, in order to ascertain if it be entire, or if it be wounded in any part. In the latter case, the orbit should be carefully examined, both with the eye and the finger; and if any portion of the tumor be seen or felt, it should be dissected away. The state of the nerve should also be examined. If this appear white, and of its natural size, a hope may be entertained that the operation will prove successful; but if it appear of a leaden color, or be altered in shape or size, there is too much reason to fear that the disease has passed beyond the part which has been removed, and that, sooner or later, a fungus will arise in the orbit, and all the old symptoms be repro-

duced. The hemorrhage consequent on the operation is seldom considerable. The arteries that supply the eye with blood are not large; and if a little time be allowed, those that are wounded will contract of themselves. It is desirable to avoid the application of lint or of any other substance within the lids, since it sometimes has given considerable pain; and, in one instance, in which the operation was performed by an eminent surgeon, it was supposed to occasion violent convulsions by its pressure against the divided end of the nerve. It is sufficient to apply over the eyelids a compress of old linen, folded six or eight times, and moistened with the liquor plumbi acetatis dilutus; and to direct the compress to be remoistened, without removing it, as often as it becomes dry. If by accident the eyelid be wounded during the operation, care should be taken to bring the divided ends together, and to confine them in their natural position either by means of sticking plaster, or of a suture with a small needle and thread. Care should also be taken, before the compress be applied, to adjust the edges of the upper and lower eyelids, so as to hinder one from lopping over the other. If, after the operation, the pain continue violent, an anodyne should be given; and, if necessary, it should be repeated after three or four hours; but its repetition, I believe, will seldom be required. Sometimes, after a week or ten days, the upper eyelid is observed to tuck in under the lower; in consequence of which the upper lashes, by rubbing against the inside of the lower lid, have been known to keep up a painful irritation. This may be obviated by fixing the end of a slip of adhesive plaster on the upper lid, and continuing it lengthways on the forehead, sufficiently tight to make a fold in the skin and hinder the edge of the lid from turning inwards. Cooling medicines, and a spare diet, are necessary for a few days; but afterwards a light preparation of cinchona, together with a nutritious diet, will be required. As the wound heals, an adhesion usually takes place between the inside of the eyelid and the bottom of the orbit; and when this happens, it is not possible to give the patient the benefit of an artificial eye, as is done after the operation for the staphyloma or the hydrophthalmia; and he must be contented either to wear a compress, bound by a riband over the orbit, or a pair of spec-

tacles, having plain glass in the ring before the good eye, and glass that is either plain, or in a slight degree opake, in that before the affected eye.

If, unfortunately, after a careful extirpation of a carcinomatous eye, a tumor again arise in the orbit, it is vain to expect benefit from a second operation, and applications of a painful kind should be avoided as much as possible. Art does not appear to be capable of doing more than to palliate the violent symptoms; by anodyne remedies; by evacuations local or general; and by tonic medicines, when the state of the general health renders these expedient.

May 28, 1810.

ON STRAMONIUM.

[From the Medical and Physical Journal for July, 1811.]

HAVING recorded the effects of the internal administration of stramonium upon the human system in health and in disease, I shall conclude with some observations upon the practice of smoking the herb, of late recommended for the cure of asthma, and other pulmonic affections.

Since the first part of this paper was written, Dr. Sims, whose attention to botanical research, amidst the engagements of extensive practice, is highly creditable, has, in a letter to the editor of the Monthly Magazine, given the following account of the introduction of smoking stramonium in this country.

"Some time in the year 1802 I received from general Gent a remedy that he had not long before brought from Madras, which, the general informed me, was used there as a specific for relieving the paroxysms of asthma, and that it was prepared from the roots of the wild purple-flowered thorn-apple, *datura ferox*. The roots had been cut into slips as soon as gathered, dried in the shade, and then beat into fibres resembling coarse hemp. The mode of using it was by smoking it in a pipe at the time of the paroxysm, either by itself or mixed with tobacco, according as the patients were previously addicted to smoking or not. General Gent procured this remedy from Dr. Ander-

son, physician general at Madras, who both re
and, I believe, used it himself.

"I happened at this time," continues Dr. S
tending the daughter of an eminent physician,
phthisis pulmonalis, combined with asthma, as appeared to me from the frequent paroxysms of difficulty of breathing, not usual in pure phthisis, at least in so early a stage of the disorder. With a view of alleviating these distressing paroxysms, I recommended a trial of this remedy, which to me was at that time perfectly new. The relief obtained was far beyond expectation; and, although gradually sinking under an incurable disease, this amiable lady continued to experience great satisfaction from its use, almost to the fatal termination."

Soon afterwards Dr. Sims recommended this remedy to Mr. Toulmin of Hackney, who had for several years suffered frequent paroxysms of asthma. He received much benefit from its use, and having soon exhausted the original stock given him by Dr. Sims, at the doctor's suggestion, he had recourse to the common thorn-apple, (*datura stramonium.*) From this he experienced nearly the same relief as from the East Indian plant. Mr. Toulmin communicated the knowledge of the remedy to the correspondent in the Monthly Magazine with the signature of Verax.*

Dr. Sims concludes his letter by observing that he has two purposes to answer by it. "In the first place it will serve to point out the history of the introduction of a remedy which promises to become an important addition to the Materia Medica," and it will prove "that the original remedy, as used in the East Indies, is not exactly the same as what is used here. It is, indeed, highly probable that both species have nearly similar virtues, but the one may, perhaps, be more efficacious than the other."

This account of the remedy is certainly favourable, and from Dr. Sims's authority many practitioners will, probably, be induced to give it a trial. We must not forget, however, that the doctor has only cited two cases in which benefit has been derived from its use, evidence is still wanting to enable us to de-

* This writer has since avowed himself to be John Sills, esq. Guildford Street.

cide upon the merits of the plant, to determine in what cases its use may be profitable, and in what cases it may be hurtful.

The following lamentable history of the fate of a gentleman,* who was instrumental in introducing the practice of smoking stramonium, is probably not known by many who are industriously encouraging the use of this composing smoke.

Dr. Gibbes of Bath being called in to attend this gentleman, found him sitting up with his head reclined on a sofa in a state of stupor. His recollection was impaired, and he seemed stunned and comatose. His pulse was scarcely to be felt, and there was no great force in the carotid artery. Upon inquiring into his case, Dr. G. found that on the preceding evening the gentleman had been very much affected with shortness of breath, and that he had smoked stramonium, the effect of which was, that his breath became perfectly easy, but at the same time he had shown symptoms of stupor. He went to bed, passed a quiet night, and arose the next morning in the heavy comatose state in which Dr. Gibbes found him on his arrival at the house. A large blister was applied to the back, purgative medicines were administered; and the urine being deficient in quantity, a draught with squills and camphor mixture was ordered.

On the second day the blister had acted, the bowels had been open, and the urine in proper quantity and of a clear good color. The mental faculties were somewhat recovered. The treatment was continued, and he was directed to take nourishment, having previously reduced himself very much with the view of alleviating the distress in his breathing. On the third day his recollection was much better, his pulse perceptible and some return of shortness of breath had occurred. From this symptom Dr. Gibbes drew a favourable opinion, as he observed that as this came on, the mental faculties seemed to improve; nor could he perceive any fulness of vessels to denote apoplexy; nor any evidence of effusion since the urine had returned to its natural quantity and color. On the fourth day he was so much reco-

* The particulars of this case were communicated to Dr. Bree by Dr. Gibbes. Delicacy to the parties alone prevents the patient's name from appearing, but the authenticity of the account may be relied upon; and the opinion of Dr. G. that smoking stramonium was the cause of death is confirmed by the testimony of Dr. Parry.

vered that he was preparing to go to Clifton; but suddenly expired after dinner, either whilst sleeping, or immediately upon awaking: Dr. Parry, who was then sent for, as being nearer than the attending physician, finding him dead when he arrived.

The patient was of a full habit, and about twelve months previous to this attack, had been affected with a cough and wheezing, from which he recovered under the care of Dr. Gibbes with ordinary expectorants. At the commencement of his last illness he smoked largely of stramonium, three or four times, without consulting any one; when the serious symptoms above related supervened, and Dr. Gibbes was called in.

My own experience of the effects of this remedy has not been encouraging; I have given it in asthma, in chronic cough attended with severe dyspnœa, and copious expectoration; and in phthisis pulmonalis. Some of the patients, whose complaints were far advanced, died, as they must have done had they not taken the medicine, which however, I did not think hastened the catastrophe. In two or three instances of dyspnœa, slight and temporary benefit was obtained, but in no one instance was the disorder materially altered in character, and the patients soon abandoned a remedy from which they derived no relief. Desirous that the properties of stramonium should be properly estimated, and to prevent any suspicion that my judgment might be influenced by my own want of success, I applied for information to some of my medical friends, whom I knew were especially conversant in the disorders in which this remedy has been said to prove beneficial. Dr. Bree, with great liberality and promptitude, has allowed me to publish the following letter containing the result of his observation on the effects of smoking stramonium in asthmatical complaints.

DEAR SIR, *May* 10, 1811.

I very willingly comply with your request that I should report the result of my observations on the efficacy or influence of stramonium in cases of asthma that have fallen under my view.

In certain cases I tried the extract of stramonium many years ago, but I was not encouraged by my experience at that time to pursue the practice of giving it in general cases of asthma.

In the last year the public were informed, by writers in journals and newspapers, that the smoking of this herb produced ease, and even effected cures in convulsive asthma. The authorities for such success were of a mixed character, some of them being satisfactory, as far as they reported benefit in the fits of asthma; but others, more numerous, were very suspicious, as they were not sanctioned by names, and most of them asserted *cures* after the use of the remedy for a very short time in this disease, of which the access of the paroxysm is both periodical and uncertain.

The evidence of advantage from smoking stramonium had a doubtful aspect to a considerate physician, and this character was not rendered more clear by the appearance of a "Familiar Treatise" on the subject, pretended to be published by Mr. Surgeon Fisher. Much of the matter in that treatise I knew to be wholly false, whilst the chief object of it was clearly displayed by the recommendation of stramonium in a *secret* composition, after the manner of other empirical nostrums.

Mr. Toulmin of Hackney gave the only testimony that deserved attention respecting the use of stramonium in asthma; but this gentleman, with the power of confuting the pretensions of others, did not offer himself to the public notice; and the same reserve, which distinguishes the professional man of science, seems to have restrained him from publishing hasty conclusions from particular facts, that are too often generalized and made subservient to unworthy purposes. I was acquainted, in a private manner, with Mr. Toulmin's use of stramonium by inhaling it, and the success which some sufferers had experienced in fits of asthma from following his practice, induced me to mention it as a possible means of obtaining relief, when other antispasmodics had been tried without effect.

From the beginning of the present year I have been more attentive to the effects of this practice.

The number of cases which I have had occasion to examine between that period, and the end of April, was 82. The patients were all disturbed in their breathing, but only a proportion of them was truly affected with convulsive asthma. To the whole number the remedy had been either useless, as regarded the removal of the disease, or it had produced injurious or fatal ef-

fects. If any signal advantage from the use of stramonium had been experienced, I should probably not have been consulted, and my report is therefore not intended to deny the success that may be asserted to have taken place in cases I have not seen. You will consider it as a faithful report respecting 82 cases of patients who had smoked this herb under various diseases, which were supposed to be asthmatic.

Those who had smoked stramonium without any permanent good effect amounted to 58. The remaining 24 had all of them been more or less injured, and some of them destroyed by the practice. I shall only mention cases which were brought to a certain state, admitting of safe inferences as to their further progress, at the end of April. They had been all of them observed with sufficient attention to enable me to ascertain, how far stramonium was capable of mitigating or removing asthma.

The first list of 58 included 11 cases of obstructed liver; these patients had lost their time in relying upon stramonium; but I do not place this inconvenience amongst the injuries derived from the practice of smoking this herb, because the constitutions of the patients were yet so vigorous, as to be capable of bearing the necessary evacuations. All of them had constant dyspnœa, and most of them had experienced paroxysms of convulsive breathing at intervals. Three were in an advanced state of the disease, having hard bellies, and swelled legs. Seven gradually recovered by the treatment that was applied for the removal of congestion in the liver, their dyspnœa leaving them, as the disease of this organ gave way. These 11 cases show the effect of advice which people, ignorant of the distinction of diseases, give with confidence to their friends without any authority excepting that of the advertisements of their newspapers.

The remainder of the 58 patients had the usual signs of the asthmatic constitution. They were generally satisfied with a plan, less miraculous in the promise of immediate cure, but more likely to restore tone to their habits, and with the assurance that relieving the convulsive paroxysm of asthma is not removing the disease. I had seen many of them before, and some of these did not refrain from complaining of the assent I had given in the winter to their trials of stramonium in the difficulty they experienced of appeasing the fit.

The 24 patients who have been stated to have suffered injury from the smoking of stramonium were all disordered in their breathing, and their dyspnœa, at intervals, assumed the form of convulsive asthma.

Of this number I shall first mention seven patients, whose symptoms indicated phthisis, and whose lungs were weak, and had been long subject to inflammatory attacks on changes of weather, and the taking of colds. The oldest of these was 35 years of age. Their habits were thin, irritable, and weak, and the pulses of all of them in their best state of a dangerous quickness. In their former attacks of difficult respiration, small bleedings, with blisters and febrifuge draughts, that gently promoted expectoration, had always afforded relief. They came under my care in March and April, and all without exception, attributed the aggravation of their complaints to the smoking of stramonium, or to the use internally of an oxymel of stramonium. Some of these patients were relieved by the same means, as had been before repeatedly applied to their cases, but three of them spat blood, after violent heat and stricture under the sternum had continued for many days. They now expectorate pus, and are greatly wasted with hectic fever and night sweats, and give no prospect of a fortunate result from any mode of treatment.

Three persons, who had passed the meridian of life, and had suffered asthmatic affections, and coughs, for many years, with great debility and emaciation of the system, experienced paralytic tremblings from smoking stramonium. Their original complaints were also generally aggravated, excepting their cough, which subsided as their weakness increased. The pulse in each of these patients was so lowered, that it became difficult to feel the beating of the artery. After abandoning their practice of smoking, which two of them had pursued every evening for two weeks, and one twice a day, for ten days; they took strengthening draughts with gentle expectorants. The cough then returned to each patient, and they all recovered their former degree of health.

A lady advanced in life, of weak constitution, and particularly feeble nerves, had been long subject to coughs and asthma. She had smoked the stramonium a few times only, and it affected her head with pain and confusion, and her stomach with sickness. She was next seized with an epileptic fit, the first she

had ever experienced. This attack was followed by three more fits of the same kind, at intervals of a few hours, and she became nearly insensible. The cough left her, the pulse became scarcely perceptible, and her mind was no longer capable of any exertion. She was not wholly unconscious of her state, but stupor and somnolency overpowered the little energy she possessed, and her stools and urine passed involuntarily. At first it appeared necessary to remove congestion from the head by cupping, leeches, and blisters. Strengthening medicines were then employed in consultation with Dr. Latham. The patient slowly recovered from this critical state, and attributed her epileptic fits, and preceding confusion of head, to the smoking of stramonium.

Four persons, all of full habits, and two of them, strictly speaking, apoplectic in their forms, smoked stramonium for the cure of dyspnœa, which they called asthma. After some days experience of this practice, one of them was still capable of coughing, but with so much pain of his head, as to indicate immediate danger. He was sixty years of age, the other three were more than fifty. They so convincingly required depletion, that I was surprised it had not been advised by the most superficial of their friends. Evacuations by bleeding and purging, removed the difficulty of breathing, and probably preserved the lives of more than one.

The smoking of stramonium has been practised by many female patients. I saw two patients, of the ages of forty-five and forty-nine, of very plethoric habits, and each of them had experienced the inconvenience which so often follows the cessation of the menses. They wheezed much, and their breathing was oppressed upon every motion of their bodies. Without taking any measure pointed out by the actual condition of their habits, and from being informed only that they had asthma, they adopted the practice of smoking stramonium, and became rapidly worse. Pneumonic inflammation affected one, and intolerable head-aches with dimness of sight attacked the other. They however obtained relief by the active application of the necessary treatment.

An elderly man, whose complicated disorders had began with obstructed liver three years before, was icteric, and ana-

sarcous, with a hard belly, and irregular pulse, and had not lain horizontally for several weeks. His respiration was laborious, and he could not leave his bed without much increased agitation. I had seen him once two weeks before; and I was called to him again in the present state. I found that he had been smoking stramonium for the last two days, and he died the night after I saw him without taking medicine.

Instances of patients in hydrothorax who had applied to the fumes of stramonium, must have occurred very often to practitioners in this town during the last three months. I have seen six cases of this kind, and I am confident that at least half of them were so quieted by the practice, the force of the circulation through the lungs was so reduced, and the irritability of the frame so far exhausted, that they died prematurely as regarded the state of the disease.

The patients who suffer injurious or fatal consequences from smoking stramonium, are chiefly those who have apoplectic or paralytic habits; young persons, affected with insidious spasmodic breathing, but who are actually consumptive; and elderly persons whose protracted complaints had ended in hydropic effusion in the chest. The effects of stramonium must be referred, as Cullen has remarked, to its narcotic power; and if it be considered how universally the practice of smoking this herb has been diffused by the exertions of selfish interest, or of ignorant enthusiasm, the mischief that health and life have suffered from its use may be conceived, but cannot be very readily estimated.

I have had reported to me many deaths from smoking stramonium, and I have verified many facts of this kind, without attending to doubtful effects in cases that might have been lost without its influence. I do not go into these cases, but have spoken only of what I have seen.

I am, dear sir, with great esteem, your most faithful

R. BREE.

Dr. Gooch, of Croydon, has kindly communicated the following cases, in three of which smoking the stramonium appears to have effected present relief.

Mr. L. 22 years old, for the last four years has had a violent difficulty of breathing, attended by wheezing and cough, which attack him suddenly when in bed, or at meals, disables him from business, and sometimes continues more than a week. It occasionally seizes him so violently that he his unable to speak, and appears to be threatened with instant suffocation. He has had much medical advice without receiving material benefit. He now smoked the thorn-apple, swallowing the saliva and smoke; by these means the fit terminates in a few minutes. He smokes every day, even when the fit does not occur. Sometimes it attacks him whilst dining in company, in which case, he retires, smokes a pipe-full, and returns to his friends breathing freely.

Mr. I. a short, fat, puffy man, about thirty-six years old, has been subject to a difficulty of breathing for twenty years. It comes on suddenly, sometimes after any strong exercise, and sometimes whilst in bed, continues several hours or days, and not unfrequently with a degree of severity that disables him from business. He has used various remedies by the advice of various practitioners, with little or no relief. For the last few months he has smoked the stem of the thorn-apple, which generally removes the difficulty of breathing within half an hour, without producing giddiness or any other unpleasant effect.

A young gentleman, about fifteen years old, came to my house to day, Sunday, March 3d, breathing with great difficulty. He has been subject to asthma as long as he can remember, and formerly scarcely passed a month without having a fit, which lasted him from two or three days to a week. For the last two years he has been free from the complaint, until about a fortnight ago, when he had a fit which continued about a day. Half an hour ago he was seized with another, which was severe. I made him sit down and smoke a pipe full of stramonium; he soon began to breath with greater freedom, and in about half an hour walked home quite well. He had never in his life been relieved so soon.

The complaint recurred in about a month with its former violence, and was again removed by smoking the stramonium.

A poor boy, about fifteen years old, has been subject to asthma for the last eight years. He is seldom free from it longer

than a fortnight. It almost always attacks him in the night, waking him with cough and wheezing, which generally last four or five days. It is sometimes produced by going into a barn, from the fine dust which is raised by threshing corn. When I first saw him he was wheezing and breathing with much difficulty. The next morning he procured some stramonium, smoked a pipe full, and remained free from complaint for several days. His mother was doubtful whether the relief was to be attributed to the remedy, as the difficulty of breathing was diminishing before it was used.

A few days afterwards, he awoke about four o'clock in the morning with violent cough and wheezing. He said that "his father got up, struck a light, and brought up stairs a pipe full of the herb; he sat up in bed and smoked it. As the spittle and smoke went down it cleared his stomach, and he laid down and slept quietly till seven o'clock, when he awoke quite well." Smoking always makes him giddy.

I saw him about a week afterwards; he had had another attack this morning and smoked a pipe full, but with less relief than before. In the evening of the same day I met him accidentally, breathing with much labour. He tried a pipe when he arrived home, but without any benefit. He has tried it several times since without any relief.

Comparing this evidence of impartial medical characters, interested in upholding the dignity of their profession, and zealous in extending its utility, with the statements of cases by patients, and individuals only *commercially* interested in the sale of the remedy, we cannot hesitate in deciding against the practice of smoking stramonium in the more severe and urgent forms of asthma, and phthisical complaints. It is not attempted to deny that relief has in some instances followed its use, but the preceding facts prove that relief to be trifling indeed, when balanced with the mischief which has been effected. Ranking the herb, as we must do, amongst the narcotic poisons, we might, *à priori*, suppose that its essential qualities being copiously applied in the diffusible form of smoke to a very large surface minutely supplied with nerves, the paroxysms of a convulsive cough might be quieted, but at the same time, fatal injury might be induced on the sensorium. That this is the case is

fearfully demonstrated by the somnolency, epilepsy, mania, and apoplexy, which have been evidently occasioned by the remedy. Again, it is proved by experiments that respiration is influenced by the brain, and ceases altogether when the functions of that organ are destroyed. Now if stramonium does not always disturb or destroy these vital and essential functions, it is only when it is not applied in sufficient quantity, when the quality is impaired, or from some peculiar idiosyncrasy of the patient.

The limits of this paper necessarily forbid a minute inquiry into the various causes of asthma; but the most narrow experience suffices to inform us, that unless these are removed, the disease will recur, however its symptoms may for a time be palliated. Asthma frequently depends upon effusion of serum or mucus in the cavities of the chest, and of the pericardium, in the bronchial tubes and air cells; upon the mal-formation of the chest; upon a diseased state of lungs; upon plethora occasioning pressure and thus impeding respiration; upon extraneous substances interrupting the natural action of the lungs, &c. all of which are most clearly and scientifically investigated and described in Dr. Bree's well known treatise on the subject. When any of these causes operate, can we rationally hope to obtain relief by destroying a series of actions induced in the system, to remove such noxious and offending agents?

Some people indeed are so tenacious of life that they seem to survive the effects of any sort of practice: in some habits nature is so indulgent, that they will recover although the treatment pursued is directly opposite to that which is dictated by reason or sanctioned by experience. Thus in fevers of a similar form and type, we see some patients recover, who have sustained the diffusible stimuli of John Brown; and others the large bleeding and drastic purgatives recommended by certain practitioners, even in the most advanced stage of the disorder.

Let then those worthy gentlemen who, from motives of mistaken humanity, have published their cases, and circulated their boasted cures throughout the empire, be cautious how they persevere in the practice which they so fearlessly recommend. If learned and skilful professional men are slow to admit dubious and dangerous remedies into their practice, surely those who have no pretensions to medical knowledge should

be on their guard, not to deceive themselves, by imagining that because they have escaped with impunity, they may at all times be so favoured, or that their friends may be equally fortunate.

Experiments to prove that Fluids pass directly from the Stomach to the Circulation of the Blood, and from thence into the Cells of the Spleen, the Gall Bladder, and Urinary Bladder, without going through the Thoracic Duct.

BY EVERARD HOME, ESQ. F.R.S.

[From the Philosophical Transactions for 1811, Part I.]

Having on a former occasion laid before the society some experiments, to prove that fluids pass directly from the cardiac portion of the stomach, so as to arrive at the circulation of the blood without going through the thoracic duct, the only known channel by which liquids can arrive there; the present experiments are brought to confirm that opinion: but in stating them, I wish to correct an error I was led into, in believing that the spleen was the channel by which they are conveyed. At the time I made my former communications, I was conscious that the facts I had ascertained were only sufficient to open a new field of inquiry; but as I might never be able to make a further progress in an investigation beset with so many difficulties, I thought it right to put them on record. Since that time I have lost no opportunity of devising new experiments to elucidate this subject; and the circumstance of Mr. Brodie, the assistant of my philosophical as well as professional labours, having tied the thoracic duct in some experiments which will come before the society, suggested to me the idea, that if the thoracic duct was tied and proper experiments made, there could be no difficulty in ascertaining whether there was any other channel between the stomach and the circulation of the blood. With this view I instituted the following experiment, which was made on the 29th of September 1810; by Mr. Brodie, assisted by Mr. William Brande and Mr. Gatcombe. I was unavoidably prevented from being present during the time of the experiment.

EXPERIMENT 1. A ligature was passed round the thoracic duct of a rabbit, just before it enters at the junction between the left jugular and subclavian veins: an ounce of strong infusion of rhubarb was then injected into the stomach. In three quarters of an hour some urine was voided, in which rhubarb was distinctly detected by the addition of potash. An hour and a quarter after the injection of the rhubarb the animal was killed: a drachm and a half of urine was found in the bladder highly tinged with rhubarb, and the usual alteration of color took place on the addition of potash. The coats of the thoracic duct had given way opposite the middle dorsal vertebra, and nearly an ounce of chyle was found effused into the cavity of the thorax, beside a considerable quantity in the cellular membrane of the posterior mediastinum. Above the ruptured part the thoracic duct was entire, much distended with chyle; and on tracing it upwards, the termination of the duct in the vein was found to be completely secured by the ligature. The lacteal and lymphatic vessels had given way in several parts of the abdomen, and chyle and lymph were extravasated underneath the peritoneum. In this and the following experiments, the infusion of rhubarb was employed in preference to the prussiate of potash, in consequence of its having been found in those I formerly made, that one drop of tincture of rhubarb could be detected in half an ounce of serum, and nothing less than a quarter of a grain of prussiate of potash in the same quantity could be made to strike a blue color when the test was added.

EXPERIMENT 2. The experiment was repeated upon a dog. In this I was assisted by Mr. Brodie, Mr. William Brande, and Mr. Clift and Mr. Gatcombe. After the thoracic duct had been secured, two ounces of a strong infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine in the bladder, on the addition of potash became deeply tinged with rhubarb. The bile in the gall bladder, by a similar test, was found to contain rhubarb. The lacteal vessels in several parts of the mesentery had burst, and chyle was extravasated into the cellular membrane,—the thoracic duct had given way in the lower part of the posterior mediastinum, and chyle extravasated. Above the ruptured

part, the thoracic duct was much distended with chyle,—it was readily traced to the ligature, by which it was completely secured.

These experiments appeared to establish the fact, that the thoracic duct was not the channel through which the infusion of rhubarb was conveyed to the circulation of the blood, and now it became easy to ascertain, whether it passed through the spleen by extirpating that organ, and repeating the last experiment.

On the 21st of October, 1810, the following experiment was made, with the assistance of Mr. Brodie, Mr. Clift, Mr. Gatcombe and Mr. Money.

Experiment 3. The thoracic duct near its termination was secured in a dog whose spleen had been removed four days before, and three ounces of infusion of rhubarb were injected into the stomach. In an hour and a half the dog was killed, and the urine was found strongly impregnated with rhubarb,—and on examination the thoracic duct was found completely secured by the ligature. Several of the lacteals had burst, but the duct itself had not given way,—it was greatly distended with chyle and lymph.

By this experiment it was completely ascertained that the spleen is not the channel through which the infusion of rhubarb is conveyed into the circulation of the blood, as I had been led to believe, and therefore the rhubarb, in my former experiments detected in the spleen, must have been deposited there in the same manner as in the urine and in the bile.

The detection of this error, made me more anxious to avoid being misled respecting the thoracic duct,—and therefore, although there was little probability that the infusion of rhubarb could have passed into the lymphatic vessels, which open into the bloodvessels of the right side of the neck, I thought it right, before I proceeded further, to repeat the experiment, securing the termination of the thoracic duct on the left side, and the lymphatic trunk on the right side, where it empties itself into the angle between the jugular and subclavian vein. This was done on the 28th of October, 1810, with the assistance of the same persons as in the last experiment.

Experiment 4. The thoracic duct of a dog was tied, as in

the former experiment; in doing it the duct was wounded, and about a drachm of chyle flowed out; the lymphatic trunk of the right side was then secured. After this, three ounces of infusion of rhubarb were injected into the stomach, and in an hour the dog was killed. The urine and the bile were found distinctly impregnated with rhubarb. On opening the thorax, some absorbent vessels, distended with lymph, were seen on the right side of the spine, entering an absorbent gland on the second dorsal vertebra, and the vasa efferentia from the gland were seen uniting with other absorbent vessels, and extending towards the right shoulder, where they formed a common trunk with the absorbents from the neck and axilla; this trunk was found included in the ligature. The thoracic duct was moderately distended with a mixture of chyle and lymph; in tracing it upwards, an opening was seen in it immediately below the ligature, through which the contents readily passed out when pressure was made on the duct; above this opening the duct was completely secured by the ligature. Nearly a drachm of the fluid contained in the thoracic duct was collected and tested by potash, but there did not appear to be any impregnation of rhubarb.

EXPERIMENT 5. The last experiment was repeated on another dog, on the 21st of January, 1811, with the assistance of Mr. Brodie, Mr. W. Brande, Mr. Clift, and Mr. Gatcombe. The dog was killed an hour after the thoracic and lymphatic trunk had been secured, and the infusion of rhubarb had been injected into the stomach.

In tying the right lymphatic trunk, a lymphatic vessel, from the thorax going to join it, was wounded, from which chyle flowed out in considerable quantity during the whole time of the experiment; a short time before the dog was killed, some of it was collected, but on testing it with potash no rhubarb was detected in it.

The urine was found impregnated with rhubarb as was also the bile from the gall bladder; but both in a less degree than in the last experiment. The lacteal vessels and mesenteric glands were much distended with chyle, and on cutting into the glands chyle flowed out in considerable quantity. Some of this was collected and tested with potash, but shewed no evi-

dence of rhubarb being contained in it. The thoracic duct was much distended; it was traced to the ligature, and was found to be completely secured. Lymphatic vessels from the right side of the posterior mediastinum were seen extending towards the ligature that had been tied on that side; they were nearly empty; and the trunk formed by the junction of these with the lymphatic vessels from the right axilla, and from the right side of the neck was seen distinctly included in the ligature.

While Mr. Brodie was tracing the thoracic duct, Mr. William Brande was making an infusion of the spleen, and showed me a section of it, in which the cells were larger, and more distinct, than I had ever seen them in a dog. There was a slight tinge of rhubarb in the infusion from the spleen. A similar infusion was made of the liver; but the quantity of blood contained in it being much greater than in the spleen, the appearance was not sufficiently distinct to decide whether it contained rhubarb or not. These experiments appear completely to establish the fact, that the rhubarb did not pass through the thoracic duct, and therefore must have got into the circulation of the blood by some other channel. They likewise completely overturn the opinion I had adopted, of the spleen being the medium by which the rhubarb had been conveyed, and show that the spleen answers some other purpose in the animal economy.

The rhubarb found in the spleen does not arrive there before it enters the circulation; it is therefore most probably afterwards deposited in the cells in the form of a secretion. That the rhubarb goes into the circulation is proved by my former experiments, in which it was detected in the splenic vein. The prussiate of potash is hardly to be discovered in the blood of a living animal, since the proportion which strikes a blue color on the addition of solution of iron, is greater than the circulating fluids can be expected to contain at any one time, as it goes off by the secretions nearly as fast as it is received into the bloodvessels. In a moderately sized ass, more than two drachms must be dissolved in the blood before its presence there can be detected.

That the fluid contained in the cells of the spleen is secreted there, is rendered highly probable, since it is most abun-

dant while the digestive organs are employed, and scarcely at all met with when the animal has been sometime without food. The great objection to this opinion is, there being no excretory duct but the lymphatic vessels of the spleen; these, however, are both larger and more numerous than in any other organ; they are found in the ass to form one common trunk, which opens into a large gland on the side of the thoracic duct, just above the receptaculum chyli; and when the quicksilver is made to pass through the branches of this gland, there is a trunk equally large on the opposite side, which makes an angle, and then terminates in the thoracic duct. This fact I ascertained at the Veterinary college, assisted by the deputy professor Mr. Sewell, and Mr. Clift.

These lymphatic vessels are equally as large as the excretory ducts of any other glands, and therefore sufficient to carry off the secretion formed in the cells of the spleen; and where a secretion is to be carried into the thoracic duct, it would be a deviation from the general economy, were any but lymphatic vessels employed for that purpose. It is a strong circumstance in favour of the secretion being so conveyed, that, in the last experiment, the lacteals and cells of the spleen were unusally turgid, being placed under similar circumstances, the thoracic duct being so full as not to receive their contents. The purposes that are answered by such a secretion from the spleen into the thoracic duct cannot at present be ascertained.

On Gouty Concretions; or Chalk Stones.

BY JAMES MOORE, ESQ.

SURGEON TO THE SECOND REGIMENT OF LIFE GUARDS.

[From the Medico-Chirurgical Transactions, Vol. I.]

There has certainly been no disease more carefully investigated than the gout. Physicians, therefore, instead of being reproached on account of this malady, deserve praise for their labours. Yet no exertions of man can command success. But there are some symptoms with which the gouty are afflicted,

the management of which belong exclusively to surgeons. Physicians are seldom competent to enter minutely into the treatment of those tumors and ulcers which are occasioned by chalk stones; and this subject has been unaccountably neglected by surgeons, in whose province they lie. It is for this reason that I have selected it. Not, however, with the pretence of fully supplying the deficiency; but with hopes that by exciting the attention of the members of this Society to these complaints, some plan of treating them may be established; instead of their being left, as at present, to each individual, to act as his sagacity, or caprice prompts him.

The concretions which form in gouty habits are usually called chalk-stones: I shall continue to employ this term, although it is now ascertained, that they are never of a calcareous nature, but are usually composed principally of the lithic acid. The analysis of these substances I leave, however, to the professed chemists, and the theory of their formation, as well as that of gout, to speculative physicians; my intention is to give such a description as is requisite for the practical surgeon; and to point out the treatment that appears to correspond with the various circumstances.

In persons afflicted with the gout, it sometimes happens that a white liquid is effused by the exhaling arteries into internal cavities. By degrees the watery and serous particles are absorbed, leaving a substance which is at first soft and clayey, and afterwards becomes hard and friable.

This effusion occurs not only during fits of gout, but likewise in the intervals; and as the extremities, particularly the hands and feet, are the principal seat of gout, it is there, that the greatest accumulations of chalk take place. Though this process is usually preceded and accompanied by inflammation, the chalk is never inclosed in a cyst, like pus in an abscess. It lies usually in the cellular membrane in the bursæ mucosæ, or in the cavities of joints: I have even seen it thrown out between the cutis and the cuticle. But as the gouty inflammation is of the erythematous kind, there is no extravasation of coagulable lymph and no new formed covering surrounding the chalk. This point is of the first importance, and explains many of the peculiarities of gout, which is generally considered as a

phlegmon. But the absence of coagulable lymph in the inflamed parts, I consider as full evidence of the inflammation being erythematous.

The chalky liquid when first secreted gives to the finger the feeling of fluctuation, and cannot be distinguished from the ordinary serous effusion of gout. But unfortunately the absorbents cannot suck up the chalky particles. The consistence of the liquid, therefore, becomes thicker and thicker, till at last nothing remains but a hard mass. When even a considerable effusion of this kind occurs, the quantity of chalk which ultimately remains is comparatively small; as by far the greatest quantity is merely serum. It therefore usually requires repeated effusions to form any great mass of chalk, and the consistency depends upon its age, and the activity of the absorbents. The quantity at last accumulated by repeated paroxysms is in some instances immense; which augments very seriously the sufferings of the gouty. The distress, however, is not owing to any irritating quality in the chalk, but to its obstructing the motion of the tendons and joints, occasioning often a complete anchylosis, and pressing and distending the surrounding parts by its bulk. It acts therefore by mechanically embarrassing the machine of the body, and not upon the living principle; for it will often remain for years in parts highly sensible, without exciting the slightest pain or inflammation.

Although these concretions are of so mild a nature, they often are the cause of extensive mischief; bursting externally, and occasioning ulcers very difficult to heal. When a violent fit of the gout attacks a chalky tumor, the appearance is frequently very alarming, the new paroxysm being accompanied with a fresh serous and chalky effusion, which added to the old deposit of chalk, occasions a prodigious swelling, the cutis when distended to the utmost opens; yet sometimes the cuticle remains entire. The chalky or serous liquid may then be seen through the semitransparent epidermis. The surrounding integuments appear of a deep red, or of a purple hue, threatening mortification; while the pain is excruciating.

At length the cuticle gives way; a discharge of serum and chalk takes place, and a remission of all the symptoms usually follow.

During the whole of this alarming process, suppuration never occurs; but soon after the opening has taken place suppuration commences; and pus and chalk are then discharged from the ulcer. There are several unexpected occurrences in the progress of such ulcerations. When an opening is formed, the whole of the chalk never escapes, and its complete evacuation is usually a very tedious process.—This is owing to its being diffused through the cellular membrane, as in the cells of a sponge. One cell must sometimes give way after another, and small portions of chalk are successively thrown out; so that months, and even years pass away, before the whole is discharged. It also frequently happens, that the orifice contracts and closes over; leaving portions of chalk underneath. This kind of cicatrix sometimes stands its ground; but more commonly breaks out again and again, to discharge chalk. Even openings into joints, which are so dangerous when occasioned by other extraneous bodies, are often attended with no serious symptoms when the joint is filled with chalk. On such an accident happening, a surgeon unacquainted with these peculiarities, might be tempted to propose large openings, or even amputation, as the only resource for hindering extensive inflammation and carious bones. But if he treats the disease mildly, he will find that no such severe plans are requisite; for the parts will probably fall into a very tranquil, or indolent state: a sore will continue for a certain period, discharging pus, and occasionally a bit of chalk, till at last the orifice will close up. Independent of the openings formed by a fit of the gout, the skin, stretched over a mass of chalk, is sometimes thinned, absorbed, and pierced by mere pressure. At other times this is effected by common inflammation and suppuration. When openings take place in these milder ways, a less quantity of chalk is usually evacuated. But this depends entirely upon the degree of inflammation. Where the suppuration is great, it naturally detaches and washes out a greater quantity of chalk.

The last peculiarity is the rarest, namely, that a dry hard piece of chalk shall pierce the skin, and remain like an excrescence, without exciting either inflammation or suppuration.

The treatment to be adopted in these various cases is next to be considered; and it must be acknowledged, that upon this

most important point our powers are extremely limited. For gouty inflammation admits of little control; and as the lymphatics are incapable of absorbing chalk, we have no means of dispersing such tumors.

The shocking appearance of a severe fit of gout, when it attacks a part in which there is an accumulation of chalk, has been already noticed. In this situation, a warm poultice is a far better application than dry wool or flannel. If there is any threatening of gangrene, the poultices ought to be of the cordial kind, into the composition of which, porter, wine, or opium should enter. Fomentations likewise sometimes give relief. If the cutis opens, yet leaves the chalky effusion confined by the cuticle only, a puncture should instantly be made. It is imprudent to touch with a lancet the organized cutis, or even to make a large opening into the cuticle, to expose internal parts in so precarious a state; nor would it be judicious at first to use pressure to squeeze out the chalk. Even a small puncture will permit some portion of the fluids to escape, and more will run out into the poultice. The tension is then removed, and the symptoms commonly improve. When the inflammation has subsided, greater freedom may be used. Some portion of the cuticle may then be removed, to facilitate the discharge; and gentle pressure may be employed.

During the violent paroxysms that have been described, if the inflamed part is threatened with gangrene, and the patient with death, the ordinary cautious treatment of gout is entirely superseded.—Bark, aromatics, volatile alkali, opium, and wine must be exhibited in doses proportioned to the danger, and to the powers of the stomach. These stimuli ought to be gradually left off, as the danger lessens.

After the violence of the fit has subsided, an ulcer frequently remains with chalk at the bottom, which renders it extremely difficult to be healed. It is bad practice to attempt the removal of the chalk by the knife. For a wound might occasion a renewal of gout, or at least a great deal of inflammation; and as the chalk is a solid substance and dispersed in separate cells, very little could be removed by the incision. Caustics employed with caution answer better; for by destroying the membrane that confines a bit of chalk, it is often enabled to escape. Mild

dressings only are to be used; stimulants, such as the hydrargyrus nitratus might excite extensive mischief; for it must always be recollected that the gouty habit is highly irritable.

It is quite impossible to prognosticate when ulcers with chalk at the bottom will heal. They sometime become a species of issue for years, and patients flatter themselves with their being beneficial. I have not, however, seen any proof of this; nor could I observe that they diminished the frequency of fits of gout.

Masses of chalk are sometimes formed on parts so inconvenient, or occasioning such deformity, that patients are anxious to get rid of them, even at some risk. On some favourable occasions, where the constitution is sound, this may be effected by destroying the skin by the kali purum. The inflammation that follows the application of caustic is seldom considerable. It, therefore, is the best method of discharging chalk. On many occasions the argentum nitratum is sufficiently powerful. After an opening is formed, the sore is to be treated as has been already explained.

These observations are not intended as a full account of all that may be done in chalky tumors, or in ulcers arising from them, as the common rules of surgery are often applicable to such cases. I have principally taken notice of the peculiarities, which demand a deviation from those rules. It may, however, be collected from these remarks, that we are quite unable to remedy many of the evils caused by chalk. When this substance is thrown into a joint, its motion is either embarrassed, or entirely lost; when it is poured into the sheath of a tendon, the movement is obstructed, and the muscle to which it belongs is rendered useless. And when it bursts externally, ulcers of long, or endless duration, are the consequence.

These irremeable disasters ought to be represented to those who are subject to gout, to induce them to submit to such restrictions in diet, and to adopt those healthful exercises, which the experience of ages have ascertained, to be the only effectual remedies for extinguishing this deplorable malady.

The proof of this does not rest upon the delusive testimony, or oaths of even respectable and disinterested men, a species of proof which is scoffed at by all who comprehend the subject, as it is found that the attestations are strongest, and the affidavits

most numerous in favour of a medical fact, just in proportion as the fact is doubtful, or false; and that the number of specifics, and the evidence in favour of them, multiply exactly in proportion to the incurableness of the disease.

To illustrate this, take common inflammation; to lessen this affection, there are a few well known remedies: while for scrophulous inflammation which is much less tractable, the number of boasted remedies is far greater; but for gouty and cancerous inflammation, the infallible specifics are innumerable.

Time alone establishes truth in medicine. A real discovery needs not the aid of oaths, nor the zealous testimony of the grateful. By not trusting to this, the over-officious perjure themselves with good faith, to the infinite detriment of their fellow-sufferers, whom they wish to relieve. No man takes the trouble to swear, that he never saw a chalk-stone in the feet of a seaman, a soldier, or a common labourer. Nor are there any affidavits annexed to cases where abstaining from fermenting drinks, abandoning animal food, and employing daily great bodily exercise, restored martyrs to the gout, to health and strength.

Such cases have occurred in all countries, and in all ages, since medical records were kept; and are authenticated by proofs far superior to the oaths of the patient and his sympathizing friends. No credit should be given to the ignorant.

But who can benefit by such examples? So strict a regimen can only be adopted by those who are reduced to it by necessity; or by the few who are capable of relinquishing present gratifications, to escape future torture.

Means of Preservation from Imminent Danger.

[From the Annual Report of the Royal Humane Society for 1811.]

I. *Captain* MANBY's *Method of Preserving Shipwrecked Mariners.*

SIR, Yarmouth, Norfolk, March 28, 1808.

THE many melancholy instances of shipwreck which are too often witnessed on every coast of Great Britain has long excited the feelings of this enlightened nation through your philanthropic institution; and, when we consider the vast number of our fellow creatures who have perished; when we reflect on the great proportion of our countrymen who are exposed to the hazard of the sea; when we remember the importance of our sailors to our commercial interest, and to our national security; it is the duty of every friend to humanity, every friend to his country, to show himself eager to diminish danger, and use all the probable means for affording help in the hour of difficulty and distress.

Animated by these sentiments, and witnessing disasters so repeatedly happening on these shores, particularly on the 18th day of February, 1807, when numerous vessels were wrecked, and all hands perished; among them his majesty's gun brig Snipe, that was stranded near the Haven Mouth, at this place, not fifty yards from the beach, notwithstanding, from the violence of the sea and strength of the storm, no communication could be procured; and sixty-seven persons perished, in the presence of their sympathizing countrymen; from this circumstance I was led to endeavour (although repeatedly told it was totally impracticable) to prevent similar distresses in future, and directed my attention to what sailors think of most consequence, the securing a communication between the shore and the vessel in distress, by the means of throwing a rope by the force of gunpowder; knowing, in the success of that expedient, there were numerous ways to rescue the unfortunate persons on board. From the melancholy day above stated I have been making innumerable experiments that appeared best calculated to promise success. I have now the opportunity and

heartfelt gratification of submitting to the consideration of your noble institution minutes of experiments, an affidavit of its success, and opinions of active and intelligent men, of the good likely to result from it to this country, and to every nation on the confines of the ocean. I have the honour to be,

Sir, with all possible respect,

Your most obedient, very humble servant,

GEORGE WILLIAM MANBY, Capt.

Barrack Master at Yarmouth.

P. S. The right honourable board of ordnance have, in consequence of my application, directed to the dangerous stations of Cromer, Happisbro', Winterton, Yarmouth, Lowestoft, and Orferness, to be provided with the means adopted by me, to give relief in future.

To the Treasurer of the Royal Humane Society.

Captain Manby has since made several successful experiments with the method of preserving the lives of shipwrecked persons by means of a rope thrown over a vessel in distress, from a mortar on shore. This rope is secured from burning by a sheath of leather round the end introduced into the mortar with the ball. The experiment was made at Lowestoft, on the 26th of August and 10th of September, before many spectators, and several persons of the Suffolk Humane Society; and the opinion of the latter has been confirmed by the united testimony of many naval officers.

" Suffolk Humane Society at Lowestoft,

Aug. 26, and Sept. 10, 1807.

Rev. I. G. Spurgeon, Vice-President, } in the Chair.
Rev. Bence Bence, Treasurer, }

On both occasions it was unanimously resolved;

" That, in the opinion of the Suffolk Humane Society, as well as of that of many other gentlemen present, captain Manby's experiments for effecting a communication between the shore and a stranded vessel, made by throwing a shot, with a rope appended to it, over a vessel, if she be near land; or by a grapnel thrown from a mortar, by which a boat can be hauled over the

surf; have completely answered the purpose for which they have been attempted.

"M. MAURICE, Secretary."

The society instituted at London for the Encouragement of Arts, Manufactures, and Commerce, voted captain Manby their gold medal in testimony of the importance of his discovery.

In addition to the foregoing information, inserted in the Report of 1809, G. L. Reed, Esq. of Hackney Grove, an active friend of this society, and who was principally instrumental, with the late Dr. Hawes, in establishing the Lowestoft Humane Society, has favoured this society with the subsequent communication.

MY DEAR FRIEND,

As every instance of benevolence affords you peculiar pleasure, I have the satisfaction to transmit one that I am sure will be highly gratifying. There are particulars not explained that might have been introduced, that would delight your affectionate daughter; such as the men on shore stripping off their dry clothes to put them on the poor wet sailors, as soon as they reached the shore, and the uncommon tenderness of the people of the inn to comfort the distressed foreigners. These particulars are omitted in the narrative, but to you will be pleasing. Captain Manby last week tried some new experiments. Two anchors were laid out 140 yards from the shore, a rope suspended between the anchors, and a buoy floating in the middle; a barbed shot was fired from a mortar with a rope of two inches and a half attached to it. The shot went over the rope fastened to the anchors. It was then hauled tight, and the resistance was found equal to pull off any boat from the beach, however rough the surf. On this plan we are about to act. You have heard of the remarkable preservation of a man by our life-boat on Dec. 13. Indeed, my friend, that day was an evidence of such exertion as I cannot sufficiently praise. Would you have any objection to lay the narrative before your honourable board? To them every such statement will be gratifying; and they may rely, that whatever reward they

bestow upon any whom they deem deserving of their notice, if transmitted to either the treasurer or secretary of the Suffolk Humane Society, will be applied precisely as they direct.

Believe me ever your obedient and sincere friend,

M. MAURICE.

Normanstone, Jan. 16, 1810.

To G. L. Reed, Esq.

The following very interesting narrative was presented to the committee of the Suffolk Humane Society, held at the Rev. J. G. Spurgeon's, Lowestoft, on Monday Jan. 15, 1810. Its importance to the public does not merely depend upon its authenticity, but also upon the knowledge of the means that have been effectual in preserving life when exposed to the greatest danger.

On Saturday Jan. 13, the hoy Elizabeth Henrietta, of Papenburgh, captain Vanderwall, from Liverpool to Rotterdam, salt laden, sprung a leak, and after 15 hours incessant toil at the pumps, the men were obliged to run the vessel on shore near the signal house in Kessingland. The distance from Lowestoft is near four miles. The wind was at east, and blew very strong. A very heavy surf was upon the shore. It was evident that unless a communication could be secured by throwing a line from the shore to the ship, according to captain Manby's judicious plan, the crew must inevitably perish. All the apparatus was at Lowestoft. Every possible exertion was applied to facilitate its removal; still, from the distance, the nature of the cliff, and the roads, unavoidable delay occurred before the mortar could be fired. In the mean time a buoy was veered from the ship, but not near enough to be reached by a grapnel. The crew consisted of the captain and seven men. The captain betook himself to the shrouds, about one-third of the way up the mast; the seven men secured themselves on the bowsprit. The deck was under water; the whole ship ready to sink. In these circumstances the mortar was fired. The shot and line reached the bowsprit, and fell in the midst of the seven men. The line was only one inch and a half in circumference; to this the seven men fastened themselves, about two yards distant from each other. They then

dropped in succession into the sea, and sunk till the line was hauled tight from the shore. Sometimes they were seen, sometimes covered with the sea. In this manner they were dragged about eighty yards through the water, and then all safely landed. Six out of the seven lowered themselves into the sea free from entanglement; the seventh by accident threw himself on the wrong side of the rope attached to the bowsprit. In this situation they would have perished had not the rope fastened to the bowsprit broken, when the line from the shore was hauled tight. The feelings and painful anxieties of the persons on the shore, who were aware of the extent of the pending calamity, can better be imagined than described. The afflictive part of the narrative remains to be stated. Captain Vanderwall was still in the shrouds, and saw all his people safe on shore. The signs he made showed the anguish of his mind; all was done for his relief that could be done. A second shot was fired, and the rope attached to it was thrown on the yard of the ship where the captain was standing. He looked earnestly at the rope, but, from some cause made no attempt to reach it. The deck was then broken up, and all communication with every other part of the ship was cut off. Another shot was fired, and the rope passed very near the unhappy sufferer. At this instant all the masts gave way, and the captain was buried in the midst of the wreck.

The greatest praise is due to the pilots and seamen of Lowestoft and Pakefield for their zeal and exertions upon this occasion; their zeal and exertions are indeed, on all occasions in which distress is witnessed, a most honourable part of their character. It would, however, be highly improper to omit the mention of Robert Rede, Joseph Denny, Charles Barrett, Thompson Swan, James Stebbings, jun. of Lowestoft; John Cunningham, of Pakefield; and John Davey, of Kessingland. The activity and perseverance shown by the above persons cannot be too highly extolled. The judicious plans and indefatigable attention of captain Hinton, of the royal navy, Mr. James Reeve, and Mr. Elph, entitle them to the highest approbation.

II. *Mr.* Thomas Cleghorn's *Method of saving Shipwrecked Mariners.*

This ingenious writer, inventor of the ice life-boat, has published in his Navigator's Life-buoy, various plans of forming navigation life-boats; in a great measure by means of empty hogsheads or casks closely bunged to confine the air. He has kindly given unlimited permission to introduce here, his useful observations and directions.

In shipwreck, where only a single empty hogshead or cask is at hand, without a boat or plank lashed to it, it would be liable to turn or roll in the water. But if two casks could be had, the best way would be to lash them together side to side, which would completely prevent them from turning, and so secured, might support twenty or thirty men in sea-water.

It may be remarked that casks lashed together, two by two, for detached parties of men, will generally be preferable to any other number of casks so lashed; because they do not turn in water like one cask, and they have a greater proportion of horizontal circumference than three, four, or any other larger number; and when casks are lashed together in pairs, men may cling to both ends, and to one side of each; but if three or more casks are lashed close together, side to side, men can only cling to the ends, and to one side of the first and last, there being no room between the sides of the intermediate casks.

Very useful rafts might frequently be made, by lashing empty casks to planks, broken masts, yards, oars, boards, or almost any other pieces of timber at hand. Thus a broken mast, yard, or plank, may have an empty cask lashed across each of its ends, and being so much heavier than the casks in water, would always be undermost, and serve as a keel. Two ropes might be fixed to the uppermost parts of the ends of the casks, and drawn tight, parallel to the keel, and to one another, for the men to hold by.

To a boat, capable of carrying only six men in moderate weather, were two hogsheads, or even only one placed inside, and strongly lashed near the middle (if two casks, one near

each end) of the boat, as high as can be done conveniently, it is presumed that at least fifty men might be supported.

Were the use of empty, or air casks, to be adopted for the purpose in view, it would be proper to have rings or hoops permanently affixed to the casks, by which men might hold, and to which ropes or cords might quickly be tied when wanted.

III. *The Cork Jacket.*

This apparatus of easy construction, consists in sewing thin flat pieces or shavings of cork, in a waistcoat or jacket to fit close to the body; and being secured by buttons or strings, will remain in a state of sufficient stability.

IV. *Mr* Mallison's *Method of applying Cork to prevent drowning.*

This ingenious method consists in adjusting two pieces of cork, so secured by tape or cords, as to be made to fit the breast and back, and to retain the positions in which they are placed; at the same time admitting a free use of the arms. The whole apparatus may be procured at a very trivial expense.

V. *Mr.* Spencer's *Invention of a Marine Spencer.*

This is made in the form of a girdle, of a diameter to fit the body, six inches broad, composed of about 500 old tavern corks strung upon a strong twine well lashed together with lay-cord, covered with canvas, and painted in oil so as to make it water proof.

Two tapes or cords, about two feet long, are fastened to the back of the girdle, with loops at the ends. There is another tape or cord, about two feet long, in the middle of which a few corks are strung, covered with canvas, and painted as above. There is a pin of hard wood, three inches long, and half an inch diameter, fastened to the front of the girdle by a tape or cord about three inches long.

When the mariner's spencer is to be used, slide from the feet close up under the arms, bring the tapes or cords one over each shoulder, and fasten them by the loops to the pin, and

bring the tape or cord between the legs, and fasten it to the other pin.

A person thus equipped, though unacquainted with swimming, may safely trust himself to the waves; for he will float head and shoulders above water in any storm, and, by paddling with his hands, may easily gain the shore.

Such a spencer may also be made of cork shavings at a very trivial expense.

VI. Chinese *Method of preventing Children from drowning.*

In this empire, in which perhaps millions of persons live almost wholly on board vessels on its numerous canals, it is customary for parents, to preserve their children from drowning in consequence of falling into the water, to tie upon the back of each child an empty gourd or calabash, well corked, with which they run about the decks of the vessels; and, if the accident of falling overboard should happen, they are preserved from sinking or drowing by the buoyancy of the calabash.

VII. Arabian *Invention of a Life Preserver.*

One of the most ancient methods of preventing drowning, perhaps coëval with navigation and piracy, is by the skin of a goat; though its advantages were too often applied to facilitate injustice and rapine. The banks of both the Euphrates and Tigris are infested with robbers, who are accustomed to swim aboard of the boats in the water, and to carry off whatever they can seize. Travellers have often been surprised at the length of the distances which the Arabs will pass floating on the water. They accomplish these voyages by means of a goat's skin, of which they sew very completely the different openings, with the exception of the skin of one of the legs, which they use as a pipe to blow up the rest of the skin, and afterwards twist and hold it very tight. After this preparation, they strip themselves naked, form a package of their clothes, and tying it on their shoulders, lay themselves flat on the goatskin, on which they float very much at their ease, paddling with their hands and feet, and smoking their pipes all the time. Not

only men, but women and girls, adopt this method of crossing the river, and make the air reecho with their songs while they are passing.

VIII. *Mr.* DANIEL's *Invention of a Life Preserver.*

The body of the machine, which is double throughout, is made of pliable water proof leather, large enough to admit its encircling the body of the wearer, whose head is to pass between two fixed straps, which rest upon the shoulder; the arms of the wearer pass through the spaces on the outside of the straps; one on each side, admitting the machine under them to encircle the body like a large hollow belt; the strap on the lower part of the machine is attached to the back of it, and by passing betwixt the thighs of the wearer, and buckling, holds the machine sufficiently firm to the body, without too much pressure under the arms. The machine being thus fixed, is inflated with air by the bearer blowing from his lungs, through a cock affixed to the machine, a sufficient quantity of air to fill the machine, which air is retained by turning the stop-cock. The machine, when filled with air, will displace a sufficient quantity of water to prevent four persons from sinking under water.

Case of Axillary Aneurism, in which the Subclavian Artery was tied behind the Clavicle.

BY THOMAS RAMSDEN,

Surgeon to the Royal Foundation of Christ's Hospital, to the Foundling, and Assistant Surgeon to St. Bartholomew's Hospital.

Extracted from Ramsden's Practical Observations on Sclerocele, Aneurism, &c. London, 1811.

This case did not prove ultimately successful; yet as all the more immediate objects of the operation were most satisfactorily obtained, I have thought it right to submit the following detail to the perusal of the profession, under a presumption that it contains several practical facts of considerable importance, not only with reference to this particular operation, but also to our future conduct in all cases of aneurism.

John Townly, a tailor, aged thirty-two years, addicted to excessive intoxication, of an unhealthy and peculiarly anxious countenance, was admitted into St. Bartholomew's hospital on Tuesday the 2d of November 1809, on account of an aneurism in the axilla of his right arm, which had been coming on he said about four months. He could not trace its origin to any accident; at first he supposed the swelling to be only a common boil, and therefore paid little attention to it, until the pulsation in the tumor and a distressing tingling sensation at the ends of his fingers, deprived him of sleep and rendered him incapable of working at his trade.

When he was received into the hospital, the prominent part of the tumor in the axilla was of the size of the half of a large orange; there was also a very considerable enlargement and distention underneath the pectoral muscle and adjacent parts, which prevented the elbow from being brought, by the distance of several inches, into contact with the side.

The temperature of both arms was alike, and the pulse in the radial artery of each of them was correspondent. After the patient had been put to bed, some blood taken from the left arm, and his bowels emptied; his pulse, which on his admission had been at 130, became less frequent; his countenance appeared

more tranquil; and he experienced some remission of the distressing sensations in the affected arm: this relief however was of short duration; the weight and incumbrance of his arm soon became more and more oppressive, and in resistance to every medical assistance his nights were again passed without sleep, and his countenance reassumed the anxiety which had characterized it, when he first presented himself for advice.

On the sixth day after his admission, his decline of health became so very evident, and the progressive elevation of the clavicle, from the increasing bulk of the tumor, was so decidedly creating additional difficulties to any future operation, that I considered it necessary to convene my colleagues, and avail myself of their opinions as to the propriety of performing the operation; when it was agreed in consultation, that as "the tumor (although increasing) did not appear immediately to endanger the life of the patient, from any probability of its bursting suddenly, it would be advisable yet to postpone the operation for the purpose of allowing the greatest possible time for the anastomosing vessels to become enlarged; and in the meanwhile that the case should be most vigilantly watched."

About this period of the case the pulsation of the radial artery of the affected arm gradually became more obscure, and soon after either ceased entirely, or, what is more probable, was lost in the succeeding edema of the fore-arm and hand, both of which became loaded to a great extent.

Notwithstanding the aneurismal tumor had continued to increase, and the patient's health had proportionately declined, yet no particular alteration was observed on the integuments until I visited him in the evening of the twelfth day after his admission, when I found him complaining of more than usual weariness and weight in the affected limb, and painfully impatient from the impossibility, as he described it, "of finding a posture for the arm."

On examining the tumor a dark spot appeared on its center, surrounded by inflammation, which threatened a more extensive destruction of the skin. Under these symptoms and appearances no farther postponement of the operation being admissible, I performed it next day in the following manner.

Of the Operation.

The patient being placed upon an operating table, with his head obliquely towards the light, and the affected arm supported by an assistant at an easy distance from the side, I made a transverse incision through the skin and platysma myoides, along and upon the upper edge of the clavicle, of about two inches and a half in length, beginning it nearest to the shoulder, and terminating its inner extremity at about half an inch within the outward edge of the sterno-cleido-mastoideus muscle. This incision divided a small superficial artery, which was directly secured. The skin above the clavicle being then pinched up between my own thumb and finger and those of an assistant, I divided it from within outwards and upwards in the line of the outward edge of the sterno-cleido-mastoideus muscle to the extent of two inches.

My object in pinching up the skin for the second incision was to expose at once the superficial veins, and by dissecting them carefully from the cellular membrane to place them out of my way without wounding them. This provision proved to be very useful, for it rendered the flow of blood during the operation very trifling comparatively with what might otherwise have been expected; and thereby enabled me with the greatest facility to bring into view those parts which were to direct me to the artery.

My assistant having now lowered the shoulder* for the purpose of placing the first incision above the clavicle (which I had designedly made along and upon that bone), I continued the dissection with my scalpel until I had distinctly brought into sight the edge of the anterior scalenus muscle, immediately below the angle, which is formed by the traversing-belly of omo-hyoideus and the edge of the sterno-cleido-mastoideus, and having placed my finger on the artery at the point where it presents itself between the scaleni, I found no difficulty in tracing it without touching any of the nerves to the lower edge

* In my first incision I intentionally cut down along and upon the clavicle, as a security against wounding any superficial vessels, a very little lowering of the shoulder therefore placed the incision in the situation I wished to have it for the purpose of proceeding with the operation.

of the upper rib, at which part I detached it with my finger nail for the purpose of applying the ligature.

Here however arose an embarrassment, which (although I was not unprepared for it) greatly exceeded my expectation. I had learned from repeatedly performing this operation many years since on the dead subject, that to pass the ligature under the subclavian artery with the needle commonly used in aneurisms would be impracticable; I had therefore provided myself with instruments of various forms and curvatures to meet the difficulty, each of which most readily conveyed the ligature underneath the artery, but would serve me no farther; for being made of solid materials and fixed into handles, they would not allow of their points being brought up again at the very short curvature which the narrowness of the space between the rib and the clavicle afforded, and which in this particular case was rendered of unusual depth by the previous elevation of the shoulder, by the tumor.

After trying various means to overcome this difficulty, a probe of ductile metal was at length handed me, which I passed under the artery, and bringing up its point with a pair of small forceps, I succeeded in passing on the ligature, and then tied the subclavian artery at the part where I had previously detached it for that purpose. The drawing of the knot was unattended with pain, the wound was closed by the dry suture, and the patient was then returned to his bed.

Copy of the Journal.

Evening visit on the day of the operation.—The distressing tingling sensation at the ends of the patient's fingers ceased from the time the ligature was applied to the artery; he already has greater facility in placing the affected arm, and is in all respects much more free from pain than he was previous to the operation; he has had some refreshing sleep; his pulse is 110; the temperature of both arms appears equal; the patient indeed says, that to his feeling he has even more warmth in the affected arm than in the other.

Morning visit second day.—The patient has passed a much more quiet night, and feels more comfortable than at any time ince his admission into the hospital; he has slept five or six

hours; his tongue and skin are moist, and his pule is less frequent. The temperature of both arms is alike, and the same as last night.

Evening visit second day.—The patient has dozed a little during the afternoon, and taken light nourishment in the course of the day; his pulse is 120, yet regular; he complains somewhat of thirst, but his skin and tongue are moist; the temperature is the same in both of his arms; the tumor is less tense; the edema of the fore-arm and hand is considerably reduced; the inflammation of the skin in the axilla has subsided; the discolored spot is not at all increased, and the patient is entirely free from pain.

Morning visit third day.—The patient did not go to sleep before five o'clock in the morning, but from that time until nine o'clock he has enjoyed a composed sleep, and says, that he feels himself much refreshed, and that he is perfectly relieved from all pain and uneasiness. The temperature of both arms continues equal and the same as last night.

Evening visit third day.—The patient makes frequent attempts to expectorate, and says that "he is inconvenienced by phlegm;" he has had sleep at intervals and is quite free from pain; the skin and tongue are not so moist as yesterday, and the pulse is a little irregular. The tension of the tumor is very much abated; the edema is nearly removed; the discolored spot in the skin of the tumor has not increased; the temperature of both arms is equal.

Morning visit fourth day.—The patient has passed a very good night, and says that he feels much more comfortable than at any period since the commencement of his complaint; he has not any pain in the wound or affected arm; the skin and tongue are moist, his pulse is 105, and both regular and soft; the discolored spot in the tumor is more darkened, but it has not extended; a small superficial crack is observable in its center; there is no difference between the arms in their temperature.

Evening visit fourth day.—The patient has taken sufficient nourishment, with wine occasionally; the discolored spot in the axilla has extended to the size of a dollar, and there is a little oozing from the crack in its center. The tension of the tumor is farther lessened, and the edema of the fore-arm and hand is entirely removed. The patient complains of thirst, his tongue

is dry, and the pulse increased to 130; the temperature in each arm is the same; he is yet incumbered with what he calls phlegm, and seems to suffer under oppression at his chest.

Morning visit, fifth day.—The patient has slept during the greater part of the night, his pulse is very quick, and at times very feeble; his tongue is parched, the skin dry and heated; the sloughing point on the tumor is more distinctly marked at its edge, but it has not extended; the oozing from its center is merely superficial. The wound is now dressed for the first time; it has a favourable appearance and is quite free from pain; the patient complains of great weariness and weight in the affected arm, and labours under considerable oppression at his chest; the temperature in both arms continues to correspond.

Visit fifth day, four o'clock P. M.—The patient about noon became restless, and betrayed symptoms of aberration of mind; he now expresses great anxiety to see his relatives, as he has something, he says, to impart to them of great importance which preys upon his mind; he has been observed during the day to place his hand frequently upon his breast; he complains of the affected arm; his pulse is very rapid and intermittent; the arms are of equal warmth; the slough in the axilla is not more separated.

Evening visit, fifth day.—The patient's wife and brother are with him; he has unburthened his mind to them, and appears to be more composed; his pulse however is too rapid to be reckoned, and intermits.

On being asked what causes him so frequently to place his hand upon his chest, he replies, "My pain, it is in my heart!" An attempt is made to cheer him with a hope of recovery; he becomes more tranquil, and expresses a wish to be raised in his bed; the assistants being unable to place him quite upright, he makes an exertion to raise himself; a strong convulsive action takes place about the region of the heart, his countenance changes, and in an instant—he expires.

Appearances after Death.

On examination of the body after death but few peculiarities presented themselves, some of them however appear to me to be well deserving our attention.

The subclavian artery, excepting at the aneurismal aperture, was in a perfectly healthy state. The arteries branching off from it, on which the limb was to be dependent for its future support, had not acquired any increase of capacity beyond that which is natural to them. The heart, and the large vessels immediately in connexion with it, were perfectly sound, but on opening the vena cava superior it was found to contain a large body of coagulable lymph, firmly adherent to its internal coat, and hanging pendulous into the auricle, where it applied itself like a valve, and totally obstructed the communication between the auricle and the ventricle.

The aneurismal tumor contained about two pints of blood, the greater part of which was in so fluid a state that it escaped through a small puncture which I made with my scalpel. The front of the tumor was covered with a strongly connected substance, bearing some resemblance to a sac, but its posterior and other boundaries were formed merely of those parts (unaltered from their healthy state) with which the effused blood had happened to come into contact.

The subclavian artery where the ligature was applied was so very nearly separated, that it only held together by a few shreds of dead matter. Each extremity of the almost divided artery, on being laid open, was found to be already completely consolidated and impervious, and no doubt could exist of its being at this early period fully competent to resist the impetus of the blood from the heart. I had also to remark at these extremities a small deposit of coagulable lymph, which was closely connected with the internal coat of the vessel, and seemed to be placed there as an additional means of securing its obliteration.

In considering the facts presented by this case, our attention is first attracted to those which offered themselves during the operation, and next to those which occurred immediately after the application of the ligature.

By the former we perceive that the tying of the subclavian artery behind the clavicle is not only practicable, but with the assistance of proper instruments almost as easy as many other

operations in surgery; from the latter we learn, that notwithstanding the circulation in the arm is intercepted in the main channel, nature in her ample provisions supplies means to support the limb through anastomosing vessels, and that too without any additional increase of their natural diameter.

This consideration is of great importance, and may guide our practice in regard to aneurismal affections in general.

It has been a received opinion, that in every case of aneurism it is prudent to defer the operation as long as possible, in order to allow time for the anastomosing branches to become enlarged previous to the main artery being obliterated; and that the chances of recovery to the patient are proportionate to the time which can be allowed for that purpose.

This opinion was acted upon in the present instance, so far as the immediate safety of the patient could permit. The operation was not performed until upwards of four months after the tumor was first discovered; at the end of which long period however, (as appeared on dissection,) no observable increase of size had taken place in any of the anastomosing vessels,* yet they proved quite equal to the support of the limb; and though the current of blood through the subclavian artery was thus cut off, the temperature of the arm did not experience even a momentary interruption.

It may farther be remarked, that the arm instead of suffering in other respects any inconvenient privation, as might have been expected, from the loss of its chief artery, not only became immediately free from pain, but so far from exhibiting any deficiency of its customary powers, was perfectly at the patient's disposal in regard to posture (though previous to the operation he could not move it at all,) and had actually recovered from the whole of its edematous appearance within forty-eight hours after the operation.

Whilst these favourable circumstances were taking place in

* When, after death, the anastomosing vessels of a limb which has at a previous time been successfully operated upon for aneurism are discovered to be enlarged, such enlargement is not to be viewed as a provision necessary to the preservation of the limb, but as a consequence which has taken place long after the limb has recovered its natural powers.

the arm, an equal improvement was observable in the general health of the patient.

On the second day we find him representing himself "as more comfortable than at any time since his admission into the hospital, enjoying sound sleep, his tongue moist, and his pulse less frequent."

On the third day he says he is much refreshed, is perfectly relieved from pain and uneasiness, and has to complain solely of some trifling inconvenience from phlegm.

On the fourth day in the morning he is yet farther improved, having passed a very good night, and feeling "much more comfortable than at any time since the commencement of his complaint."

Thus far, therefore, we distinctly see the system availing itself of the great relief afforded by the operation, and making every effort toward recovery. But in the evening of the fourth day the scene is materially changed.

Notwithstanding the patient had taken sufficient nourishment, we find the sloughing on the tumor extended in size; he is thirsty, his tongue is parched, and the pulse is considerably quickened; the temperature of the arms indeed still continues correspondent; but he complains more of phlegm, and obviously labours under great oppression in the region of the chest.

On the morning of the fifth day, many of these unfavourable symptoms are increased, but above all the distressing oppression at the chest, which he emphatically describes by placing his hand on the part, and by exclaiming, "Here, here is my pain, it is in my heart."

As the day advances the pulse become too rapid to be reckoned, and is intermittent; the circulation about the heart appears more and more laborious; and in attempting in the evening to raise himself up in bed, a violent convulsive action about the chest takes place, and he instantly—expires.

I have already stated that a considerable substance of coagulable lymph was found consolidated within the inner coat of the vena cava superior, which hung pendulous into the heart, and seemed to form a complete barrier between the auricle and the ventricle. The accumulation of this substance I consider as the immediate cause of death.

I am well aware I am hazarding this opinion in opposition to that of many professional men, whose acquaintance with anatomy, and whose knowledge in physiology justly entitle their sentiments to every respectful attention; yet, on this occasion, I venture to dissent from them.

Substances of coagulable lymph found in the larger vessels of the dead subject, are very generally believed to be separations from the blood, which take place after death, and they are therefore at no time admitted as a probable or possible cause of dissolution.

But I do not think it unreasonable to suppose (under certain irregularities of the circulation, or circumstances by which it is temporarily retarded,) that the coagulable lymph may be separated and become attached to the internal surface of any of the large vessels or the cavities of the heart, even during the vital course of the blood. Such a substance being pendulous and at first trifling, would accommodate itself to the passage of the blood, and for a while create no other inconvenience than occasional embarrassment about the chest; but when it has acquired geater bulk and length it would become liable to be placed in the way of the circulation, and so prove a mechanical and immediate cause of death.*

We observe the coagulable lymph to be separated and to accumulate at that part of an artery which is rendered unequal by disease: this fact is obvious in every aneurismal tumor: may not therefore such an occurrence take place when the free flow of blood through the large vessels is hindered by other causes?

In the present case small consolidated bodies of coagulable lymph were found deposited within each extremity of the disunited subclavian artery, which were so firmly attached to the

* I do not mean to say that bodies of coagulable lymph do never separate from the blood subsequent to dissolution, yet I think that many of such substances found in the larger vessels after death, may be gravitations which have taken place during the latter hours of nearly exhausted life. I apprehend also that those substances of coagulable lymph which are usually met with in the dead subject, although they may be slightly adherent to the surface of the vessel, are never found to be so firmly consolidated with it as I have represented that substance to have been, to which I referred in this particular case.

internal surface, and so peculiarly placed, that it is impossible they could have been deposited after death.

I have opened many children, who in apparent health had been suddenly seized with a convulsive action of the heart, and instantly expired (such cases are by no means uncommon); and I have invariably found in children who died under such circumstances, a substance of consolidated lymph, pendulous from some part of the cavities or large vessels of the heart, and like that described in the present case obstructing the channels of its circulation.

Although I consider the substance in the vena cava of this aneurismal patient as the immediate cause of his death (which opinion appears to me to be strengthened by the previous progress of oppression about the chest, and the peculiar manner in which he died, at the instant of attempting to raise himself in his bed, at which moment I conceive the pendulous extremity of the substance to have dropped into the auricle,) I am by no means disposed to say that he would have recovered if such an occurrence had not taken place.

I am indeed persuaded that he would not; because as the integuments over the aneurismal tumor had begun to slough and that process was in progression, the enormous cavity of that tumor would very soon have been exposed, and constituted a description of wound, which a patient, so very much reduced as this poor man was by the long continuance of the disease, could not possibly have survived.

Had this case however been operated upon at the commencement of the aneurismal affection, before so severe an impression had been made on the general state of health, before the tumor had acquired such magnitude, and before the skin covering it had taken on a diseased action, every past circumstance tends to persuade me that the patient might at this time have been a living example of the utility of well-timed surgery.

As I am not aware, though the operation has been attempted, that the subclavian artery has ever before been tied in the living subject at the point where it passes over the upper rib behind the clavicle, I trust I shall be excused for having so particularly described the manner in which I conducted the dis-

section, especially since the simple provision I have laid down for avoiding the superficial hemorrhage from the veins must necessarily secure the operator from the inconvenience of having the wound constantly filled with blood. I am well aware that in the present system of operating, the pinching up the skin is not held in any estimation; but as I do not feel myself authorized, for the sake of exhibiting a semblance of expertness, to do that which will prolong the sufferings of my patient, I must acknowledge I am not yet altogether a convert to its total exclusion.*

* For some farther remarks on this case, and for plates illustrating the aneurismal affection and the instruments employed in the operation, we refer to Ramsden's Practical Observations.

ORIGINAL PAPERS.

Experiments and Observations on the Lytta Vittata.

BY GEORGE S. SCHOTT, M. D.

In the summer of 1799, immense swarms of a peculiar insect were observed covering the potatoe patches in the vicinity of this city. Their numbers indeed were so great, as to destroy all the potatoe vines in a short time.

Several accidental observations excited a suspicion that they were vesicatories. This suspicion was strengthened by the result of a few experiments, and the fact was subsequently confirmed by Dr. Chapman of Bucks county, in this State, who finding by an extensive experience, that they answered all the purposes of the cantharides, published an account of their utility, in the New York Medical Repository, and gave them the name of *Meloe Americana.*

Quantities of the insect were collected at that time, and they were to be found, for a year or two afterwards, in the shops of our apothecaries. They, however, soon fell into disuse, and have since become almost entirely neglected.

The *Potatoe Fly* belongs to the genus *Lytta*, and is the *Lytta Vittata* of Turton and Fabricius.

The characters of the genus Lytta, are,

"Antennæ filiform; feelers four, unequal, the hind ones elevate; thorax roundish; head gibbous, inflected; shells soft, flexile, as long as the abdomen."

Turton's description of the *Lytta Vittata* does not answer completely to the *Potatoe Fly*, but is sufficiently accurate to designate it as the species he intended. It is,

"*Lytta Vittata*—shells black, with a yellow fillet and margin, inhabits America. Cantharis Vittata, Oliv: Inst: Tab: 1. Fig: 3.

"Head yellowish; crown red, with two black spots; thorax black, with three yellow lines; abdomen and legs black."

The following is a description made from several very perfect specimens of the Fly.

Antennæ black; head reddish brown; two dark brown spots on the crown; elytra dark brown, with a longitudinal yellow band in the middle, margined with yellow; thorax nearly round, of a dark brown color, with three longitudinal yellow bands in some, in others the bands are wanting; legs and abdomen black; abdomen covered with a cineritious down.

These insects have hitherto been supposed to inhabit chiefly the common potatoe* and beet;† but the notion I believe is incorrect, and their scarcity for several years past, has probably been owing to this mistake. They certainly are to be met with on other plants as frequently as those just mentioned. We are assured by Dr. Barton, that they live upon the pea, the bean, the mallows, the black snake root, and upon a great number of other plants of our own gardens, fields and woods; and that their vesicating property does not at all depend upon, neither is it influenced, or in any way altered by, the plant upon which they have been feeding.‡

When the fact of their inhabiting other plants, as well as the potatoe and beet is more generally known, I have not a doubt but that a sufficient quantity may be secured every August and September, which is the proper period for collecting them, to supply the demand of one year, and in some seasons a supply competent to serve several years, might be obtained.

In the preservation of these flies the ensuing process should be adopted.

Immediately after they are caught, which can easily be effected, as they seldom use their wings, they are to be killed, by subjecting them, tied in a cloth, to the steam of boiling vinegar or water; or they may be put into a bottle, which is to be immersed in boiling water. They are then to be spread out, covered with paper, and exposed to the sun till perfectly dry, which will be in one two or three days. Particular attention should be paid to this circumstance before they are put up for use, as they soon become putrid and spoil, when moist.

With these flies, I have experimented to a considerable ex-

* Solanum tuberosum. † Beta vulgaris.

‡ See Dr. Barton's Collections for a Materia Medica.

tent, and though the parcel subjected to trial had been in the shop of an apothecary for several years, and some of them were reduced to a powder, by the decomposition which is effected in them by time, yet, in every instance they excited vesication much quicker than cantharides applied at the same moment.

An extract made from a spirituous infusion of these insects, caused a blister in the space of seven hours.

A decoction in spirits of turpentine, prepared in the same way as Dr. Hartshorne recommends with regard to the Spanish fly,* produced vesication in a few minutes, by rubbing the fluid lightly into the skin, with a piece of flannel.

A plaster of these flies, applied to an adult, in the usual manner, raised very complete vesications in four hours.

The powder, sifted from among those that were in a state of decomposition, excited a fine blister, in five hours and an half, on my own arm.

In many cases, I have applied these flies to children, with the effect of uniformly occasioning vesication, in two, and three hours.

I have also very frequently applied them to adults, and have never found it necessary to let them remain on longer than from four to seven hours.

In short, from my own experiments and observations, corroborated by information which I have derived from a variety of sources, I have no hesitation in asserting that the *Lytta Vittata* act more promptly, and with greater certainty, as a vesicatory, and will retain, in activity, their epispastic quality for a much longer period of time than the *Lytta Vesicatorium;* and that, in every form, they are superior to the cantharides, and ought therefore to be introduced more generally into practice, and especially into that of *American physicians.*

Philadelphia, Dec. 1, 1811.

* See the first number of this Journal.

An Account of the Appearances on Dissection of several Scrofulous Subjects with a few Observations on the Connexion between Scrofula and Phthisis Pulmonalis.

BY JOSEPH PARRISH, M. D.

In the course of practice, more especially among the poor in the Philadelphia Dispensary, I have frequently witnessed the fatal termination of a disease which I shall call *scrofula interna;* and I have also been permitted in some instances to ascertain by dissection the morbid appearances.

The disease to which I allude, I have more frequently observed among people of colour than whites, but the latter are not exempt. It is commonly to be met with among children, seldom above four years of age, and I have known it prove fatal in an infant of three months.

To enter into a minute detail of every symptom may not be requisite. The most prominent are, cough, accompanied with dyspnœa, and fever assuming the hectic character; in some instances, occasional diarrhœa; a tumid abdomen, with great emaciation of the face and extremities; but this is not invariably the case. These form the general character of a complaint under which patients often linger for several months, when death kindly closes the scene. Among many persons, the symptoms are referred to worms, and the little sufferers are often very improperly subjected to the long continued use of various vermifuge preparations.

My attention was particularly directed to the disease shortly before the conclusion of my studies, in consequence of visiting a child of colour near the residence of my respected preceptor. The following appearances presented on dissection.

Abdomen.

The mesenteric glands were greatly enlarged, and contained thick purulent matter; the spleen was covered with tubercles.

Thorax.

The right lung was found adhering to the diaphragm; its superior part was of a whitish cast, and when cut into abound-

ed with thick pus; a part of its middle portion appeared to have been entirely absorbed, and about one ounce of thin pus was found in the cavity; the whole texture of the lung was so completely indurated that it must have been totally inadequate to the performance of its functions for a considerable time previously to the death of the patient.

The left lung was also diseased, and especially towards its superior part; it abounded with numerous hard tubercles about the size of a pea.

S. M., a man of colour, requested me to examine the body of his child, aged about three years. It had been gradually declining for several months with cough. I believe no physician attended it through the greater part of its illness; the father informed me he had lost another child exactly in the same way, and he supposed their death to be occasioned by worms.

DISSECTION.

Abdomen.

The mesenteric glands were enormously enlarged; some were equal in size to a walnut. On cutting into a number of them, they were solid throughout. Not a single worm was to be found in the whole extent of the intestinal canal.

Thorax.

Lungs diseased; right more so than the left; the former was filled with abscesses and abounded with purulent and caseous matter: it must have been unfitted for respiration for a considerable time; the latter also contained numerous tubercles, but they had not run on to suppuration. On comparing their structure with the enlarged mesenteric glands, they appeared similar.

A few months after this I examined the bodies of two children of colour who died with the disease.

Purvis, aged twenty-two months.

DISSECTION.

Abdomen.

Liver appeared enlarged, and its convex surface was adhering to the parieties of the abdomen; spleen was studded with tubercles; mesenteric glands slightly diseased; pancreas indurated.

Thorax.

Lungs were filled with tubercles; some of them had suppurated, and abscesses were formed.

Johnson; age not noted; supposed to be younger than Purvis.

Appearances were very similar to Purvis's, particularly the spleen; the lungs were filled with tubercles, but they were not so far advanced towards suppuration.

These dissections are calculated to show that the abdominal viscera and lungs are affected by scrofula; but in the following case I trust it will appear that not only these parts, but even the heart itself is subjected to this destructive malady.

G. H., the son of one of my particular friends, was affected soon after birth with a hard, dry cough; it was so peculiar that I never met with a case resembling it. Any person who heard the cough without seeing the infant might have concluded that it proceeded from a robust adult.

When he was about three months old, I attended him with a violent attack of pneumonia; his situation was almost hopeless; but a large blister over the breast produced an unexpected alteration for the better, and he recovered from a state of immediate danger; but his cough continued, and his short life presented little else but a scene of suffering. He was often afflicted with most violent pain resembling colic, for which his parents were under the necessity of giving him large doses of laudanum. A few weeks previously to his death he was removed into the country, but his cough and hectic symptoms

went on increasing; he had frequent vomiting, and from the account of his intelligent mother, he discharged purulent matter from his lungs.

A few days before his death he was seized with convulsions, and a few hours before he died I visited him; he was quite insensible; pupils greatly dilated. He resembled a patient in hydrocephalus; his pulse was unusually irritated and remarkably tense. He was about two years old, of a very light complexion, with fine, flaxen hair.*

Assisted by my friend Dr. Samuel Tucker, of Burlington, N. J., I made the following dissection.

Abdomen.

Mesenteric glands were enlarged; not a single worm to be found in the intestines. They were examined with great attention, inasmuch as one of his female friends seemed confident that his complaints originated from worms.

Thorax.

A considerable portion of the right lung was found adhering to the pleura costalis. It abounded with tubercles containing a caseous matter. Small abscesses were formed in some parts of the lung.

The pericardium was closely adhering to the heart, and when separated from it, the whole external surface of the ventricles was found covered with numerous small distinct points or tubercles of a whitish appearance. On cutting into the ventricles, they appeared nâtural; also the valves of the heart and cordæ tendinæ.

These tubercles were very different from the crust or covering produced during acute inflammation by an effusion of coagulating lymph; because I was acquainted with that appearance, having seen it lining the thorax, and in one instance covering the heart and the internal surface of the pericardium; but these tubercles were distinct and immoveable, being examined

* This child was frequently afflicted with ulceration behind his ears. As it was very troublesome, applications were made for the purpose of healing it; when this was effected, an increase of indisposition was so obviously the result, that I determined on permitting it to remain undisturbed.

with close attention. I feel no hesitation in expressing a belief that they were of scrofulous origin. The cranium was not opened.

To be compelled by the calls of professional duty to witness the fatal issue of a disease which only admitted of palliative treatment was really painful; yet it has had a tendency to awaken attention to some circumstances which appear calculated to shed light on the subject; and even if it should appear that my reflections are already anticipated, yet still I am willing to make a diffident attempt to offer some views to my medical brethren either for their correction or approbation.

1. When scrofula makes its appearance on the external parts of the body, does it not happen that in a large proportion of cases the glands about the neck are affected by it?

2. In some diseases, gout for example, do we not often observe a metastasis? and when it changes from an external to an internal part, it is productive of danger; hence this part of practice is so well understood, that physicians in general refrain from the use of such means as are capable of dislodging the disease from parts which are not esteemed vital.

3. When such metastasis has occurred, are not sinapisms directed to the extremities, in order if possible to excite the disease therein? or even if a vital part be primarily affected is not the same practice to the extremities adopted?

From observation I am induced to believe that the same thing may occur in scrofula, only in a more gradual way. In support of this I will just state, that during the last winter I was called to a child of R. W. a few days before its death with this disease. The mother informed me, that from the birth of the child it had been subject to cough and difficult breathing: a swelling having occurred about the glands of the neck, poultices were applied, but as it did not suppurate, mercurial ointment was rubbed on, and the tumor disappeared. After this, all her symptoms were aggravated, and the disease baffled every effort to arrest it.

I once attended a little girl who had a large abscess situated on the superior part of the thorax; the discharge was great, and the debility which followed was considerable. I directed a generous diet, with tonics; and under this treatment, with

proper applications to the abscess, it healed. This child fell a victim to scrofula interna, in less than a year afterward.

These cases may tend to show a translation of scrofula from external to internal parts. I now wish to prove the reverse of this proposition, and for the purpose will offer an example.

Hetty Killen, aged about thirty years, a woman of colour, came under my notice as a dispensary patient in the early part of the fourth month (April), 1809. She had been afflicted with cough for about twelve months, and was often unwell during that period; but although in an infirm state, she continued in a family at service until the 1st of 3d month (March), when her complaints increased to such a degree that she was compelled to leave her place. At the time I visited her, she was labouring under cough, frequent chills and fever, and I believed her disease to be phthisis pulmonalis in an incipient state.

The first time I saw her, she called my attention to several large scrofulous tumors situated on the thorax, which gave her a great deal of pain, and for which she was desirous of obtaining relief.

As my attention had for a considerable time been particularly directed to the subject, I viewed her case as uncommonly interesting, and determined on giving a trial to the efficacy of the external disease in removing the internal. Emollient applications were ordered to the tumefied parts, while palliative remedies were prescribed for the cough. Suppuration took place, and the abscesses were opened. A copious discharge was established, and happily for the patient there was an evident decline of the pulmonary complaint. I endeavoured to impress her with the absolute necessity of keeping the abscesses open, and she appeared convinced of the correctness of the practice.

Her recovery was not rapid, but has proved certain. After the pulmonary symptoms declined, she was several times afflicted with diarrhœa, and frequently had chills and fever, for which bark was occasionally employed.

The discharge from the abscesses continued until last fall, when her health appearing completely reestablished, she permitted the ulcers to heal.

I visited her on the 30th of last month, and found her in fine health. She informed me that last spring she was indisposed, and one of the abscesses opened afresh, and is now running; it is situated on the left side of the thorax, near the axilla; when she is the least unwell, the discharge from it is always increased.

I believe instances may be found on medical record of similar recoveries from phthisis pulmonalis.

Now I am induced to conclude that several important practical inferences may be drawn from these facts. In the first place they point out the danger of dislodging external scrofula; and I believe that this rule in practice should not be confined to children, but is capable of application to patients of maturer age, more especially at critical periods of life; and I can conceive of no situation wherein it should be more carefully observed than in a young and delicate female, just about the establishment of the catamenia, and who inherits a predisposition to phthisis pulmonalis, which, I believe, may be styled scrofula interna, in a large majority of cases.

Would it not be safer, under such circumstances, to bear the disease with patience, rather than incur the risk of its falling on vital parts. At any rate it ought never to be attempted, unless an artificial drain by issue or seton is substituted about the thorax or on the arm.

The case of Hetty Killen furnishes an instance of the salutary operation of nature in removing an internal disease by exciting an external one.

In further confirmation of this fact, I may just observe, that in the family of R. W. I have since been called to the youngest child labouring under cough and dyspnœa. A large scrofulous tumor formed on the side of the neck. Here, as in Hetty Killen's case, I was determined to depend on the external disease for the cure. I explained my views to the parents, who cheerfully concurred with me in endeavouring to establish a drain from the tumor. This was done, and a discharge kept up for several weeks, when it declined. Soon after this another very large tumor formed on the opposite side of the neck, and was treated in a similar manner. The cough and dyspnœa subsided, and for a time, at least, this dangerous disease has been suspended. I lately visited the child to ascertain its present state.

It appeared lively and in good health. It still has a sore behind the ear, which discharges considerably, and which I have desired the mother to keep open.*

Believing, as I do, in the general inefficacy of every mode of treatment, after the disease is completely formed, it has impressed the necessity of a resort to proper treatment as soon as the slightest symptoms are evident. In confirmation of this, it may be observed, that I have been twice called to another child of S. M. with dyspnœa and some cough, and the progress of the disease was checked each time by maintaining a discharge from blisters† on both sides of the thorax. Here the agency of art in suspending symptoms seems apparent. In the cases of Hetty Killen and R. W.'s child, the physician was only the patient attendant on the operations of nature.

Reasoning from these two last mentioned cases, would it be too hypothetical so far to imitate nature in our practice, as to endeavour, in the very commencement of this formidable disease to produce tumefaction and suppuration in the glands about the neck and on the thorax near the axilla, where, in the former especially, external scrofula is so generally seated.

I confess it has been so clear to my mind that I made the proposal to a poor woman in the incipient state of phthisis pulmonalis, whose case appeared peculiarly favourable for such a practice; because from her own account she had suffered in the early part of her life very severely with scrofulous sores about her neck, which had for a long time resisted the skill of several very respectable physicians; but she declined a compliance.

Would this attempt to excite disease in these parts, which (if the expression be allowable) external scrofula chooses for its seat, be more irrational than the application of sinapisms to the lower extremities in irregular and retrocedent gout?

To conclude, may I be permitted to inquire whether the good old practice of issues and setons has not fallen too much into disuse. Do not the spontaneous cures of pulmonary consumption which sometimes occur, very forcibly inculcate their utility?

Philadelphia, 6th month, 4th, 1811.

* This little patient died afterwards with cholera.

† This child has since fallen a victim to the disease.

SELECTED REVIEWS.

Essay on some of the Stages of the Operation of Cutting for the Stone. Illustrated with an Engraving. By CHARLES BRANDON TRYE, F. R. S. 8vo. London, 1811. pp. 49. Callow.

[From the Medical and Physical Journal for July, 1811.]

THE objections which have lately been urged against the employment of the *Gorget* in Lithotomy, and the recommendation of the *Scalpel* for dividing the integuments, muscles, urethra, and prostate, have induced Mr. Trye to become the advocate of the former instrument. The objections to the *Gorget* are distinctly stated from Mr. Allan's publication; and to each objection Mr. Trye annexes an answer.

"Obj. 1. 'The beak of the gorget is apt to slip from the groove of the staff, before it reaches the neck of the bladder. This has often happened in the hands of good operators, and assuredly will happen.'

"Answ. If this happens, the staff and beak of the gorget must be both very ill made, or the surgeon must enter the urethra too soon, for instance, in the bulbous portion; whereas, he ought never to pierce that canal but in the membranous portion, and within half an inch of the prostate gland; and as then the gorget will not have more than the third of an inch to traverse, before it comes to the prostate, it will scarcely lose its way in this distance, and in the hands of a good surgeon the handle of the gorget is always depressed, in proportion as he depresses the handle of the staff."

"Obj. 2. 'If the operator presses his gorget onwards too horizontally, the prostate gland, being movable, will recede, the gorget slip from the groove, and be driven between the rectum and the bladder.'

"Answ. It is here assumed that the surgeon operates with a very dull edge to his gorget; whereas it should always make its way as easily as a scalpel would do; as will be the case, if its shape be good and its edge keen and smooth."

"Obj. 3. 'If the surgeon, or assistant, depresses the handle of the staff too much over the right groin, with the idea of

making its bend or heel be distinctly felt in the left side of the perineum, the point of the staff will slip out of the bladder, and when the surgeon has completed his external incisions, it will start through the membranous part of the urethra; and, in this case, pushing his gorget by this false guide, he will drive it between the bladder and rectum.'

"Answ. If the staff be started through the membranous part of the urethra, it will also misguide the knife of the operator, so that this is no specific objection to the gorget, but only to an awkward surgeon, using an ill contrived staff, and having an awkward assistant. However, I hope I have freed the staff from being liable to its share of this censure."

"Obj. 4. 'It is uniformly acknowledged by the best surgeons, that the gorget cuts the prostate gland very imperfectly. Its incision sometimes admits, with difficulty, the introduction of the forceps; and if the stone be large, is quite inadquate to its extraction, without dreadful laceration.'

"Answ. If the gorget does not divide the prostate sufficiently, the fault is in its make, not the principle of using it. It is not necessary nor expedient that the wound, through which the stone is to pass from the bladder, should be of the same length as the longest diameter of the stone.

"Sir James Earl has given plates of the very large stones which he has successfully extracted after using the gorget; and larger than those will rarely occur to any operator; and after all, if the surgeon finds he has a stone too large to be extracted without dreadful laceration, he can enlarge the wound of his gorget by his scalpel, just as well as if he had never used the gorget at all; for we cannot suppose, that a prudent surgeon, even if he confines himself to the knife, will make his incision with his knife of the greatest possible size, before he has ascertained that the stone is of an extraordinary magnitude; which he cannot certainly do, till he has got his forceps and finger into the bladder. Neither the staff, nor feeling through the medium of the rectum, will enable him to ascertain the size of the stone."

"Obj. 5. 'If the cutting part of the instrument be made broad, to provide against the last accident, it enters the pubis with great difficulty, grates the bone, by which the pubic artery

is sure to be wounded, and the patient brought into great danger by the hemorrhage, which commonly proves fatal.'

"Answ. I admit the objection; but I should be sorry to see any surgeon operate with a gorget, which justified the expectation of its doing so much mischief."

"Obj. 6. 'Whenever the gorget enters the bladder, the patient feels an irresistible inclination to bear downwards, by which the fundus of the bladder is pressed against the point of the instrument; and if it be kept long in the bladder, to serve as a conductor to the forceps, this generally happens.'

"Answ. I never felt this descent of the fundus in using the prostatome; and my finger has always been so instantaneously introduced along its channel, that it could not have happened without my perceiving it: and, as I never keep this instrument long in the bladder, nor use it as a conductor to the forceps, at all events, I obviate in practice this part of the objection, whatever its force may be."

"Obj. 7. 'It is possible for a rash surgeon to push the gorget on with such violence, as to transfix the bladder.'

"Answ. There is no guarding against the mischiefs of rashness, whether a man use the knife or any other invention. However, if a square stop terminate the groove of the staff, as in that which is here delineated, it must be a very rash surgeon indeed, using a very strong hand in a very violent hurry, who, notwithstanding this defence, can pierce the opposite side of the bladder."

"Obj. 8. 'When the gorget has been successfully introduced into the bladder, and all these dangers have been avoided which we enumerated, unless the operator be very careful in withdrawing it in the very position in which it enters, it will make another incision.'

"Answ. This accident is very effectually prevented by me, and, I suppose, by all operators who do not introduce the forceps on the gorget, by withdrawing it with its cutting edge under the fore finger of the left hand, which certainly keeps the bladder from coming in contact with it, while the instrument is being withdrawn by the right hand."

"Obj. 9. 'No man of feeling ever witnessed the *plunge* of

the gorget, when *driven* into the bladder, without horror, or did it without reluctance.'

"Answ. To this appeal to our sensibility I must briefly reply, that the gorget, or whatever instrument be used, should never, in its introduction, excite the idea of *driving* or *plunging*. The operator should carry forwards his instruments as coolly, deliberately, and with as much manifest command of his hand, as he would in bleeding over the basilic vein, with the artery placed immediately under it."

The character, extensive practice, and success of Mr. Trye, give considerable value to his opinions, which are unequivocally in favour of the gorget, the form of which, however, he has altered in some particulars, and has described the altered instrument under the name *prostatome.* A plate renders this description perfectly intelligible, as well as some peculiarities in the staff and forceps which Mr. Trye uses in his operations.

Having thus replied to the objections brought against the gorget, the advantages said to arise from employing the scalpel exclusively are examined.

"Is it probable," Mr. Trye asks, (p. 16.) "that the prostate will be divided with equal precision, as to direction and dimensions, and that the adjacent parts will be equally protected, if we reject every instrument besides the scalpel? I know before hand the size of the wound which the prostatome produces, and by observing the relative position of the staff, in what direction it will be made. For as the handle of the staff is inclined more or less to the right groin of the patient, so will the wound in the prostate deviate, more or less, from a right angle with the wound in the urethra. How many surgeons are there, possessed of such a delicacy of touch as to conclude the work with the knife, with equal exactness? The knife cannot here be directed by the grove of the staff, as it is, in other cases by the groove of the common director. For the staff, in dividing the prostate, is merely a goal from which the knife must set out, and to which it is to return. During the immediate act of dividing, it must move at some distance from the staff; that is to say, if the prostate be cut by a wound, forming more or less a right angle with the wound through the integuments, muscles, and urethra, which, to ensure the safety of the rectum,

must be always done. Mr. Allan's description of the operation by the knife alone, as well as his plate of the lateral incision of the prostate, demonstrate the correctness of my statement. (*See Allan on Lithotomy.*)

"'Tis true that, in the living body, the feel of the prostate is very different from that of every thing beside in its vicinity; and every part of it may be distinguished by an experienced touch. But the finger will rarely have attained that advantage the first time the surgeon is called to cut for the stone; it cannot have acquired it by exercise on the dead body, in which the feel excites a different sensation from what it does applied to living parts. Whereas if a surgeon, having that anatomical knowledge, without which no man deserves the name of a surgeon, is cool and steady, and in the habit of using instruments, and attends to rules, he will, in his first operation,* equally as in subsequent ones, make his way correctly into the bladder. He is guarded against wounding the rectum, vesiculæ seminalis, and seminal ducts, all exposed to injury, even from the most skilful hands, provided with the knife alone."

"There are two circumstances which appear very unfavourable to cutting with the knife alone: the patient being a very large and tall man, and the patient being a very small child. I have this day, March 8th, 1809, operated on a very tall man, sixty three years of age, and while I was dividing the urethra, I paid particular attention to the prostate. I am convinced, though I am tolerably accustomed to the use of the knife, and not very deficient in anatomical knowledge, that if I had attempted to have divided the prostate with the knife, I should have certainly been embarrassed by the great depth of the prostate, nor have perfected my task with accuracy.

"As, in early childhood, the prostate is too small to be felt, we want its guidance as to the situation and extent of the incision to be made by the knife. Whereas by our previous consideration of the size, and other circumstances of the prostatome, we can predetermine the situation and dimensions of the finishing wound."

* If he employs the prostatome, we presume the author means.

We shall conclude our analysis of this short pamphlet with quoting from it the following singular case of *Ischuria Vesicalis*, subsequent to lithotomy.

"A farmer seventy-five years of age, six feet in height, having been long afflicted with a stone in the bladder, in other respects in good health, submitted to lithotomy last November.

"The stone was small, and was extracted without difficulty. He was free from unusual pain till the second night, which he passed very uncomfortably, by reason of severe pain about the wound and along the urethra; the following day it was worse, and extended even to the hypogastric region. From circumstances, it was evident that he was free from peritonitis, but that the bladder was suffering from being distended with urine. A female catheter being passed through the wound, did not reach the retained water: a caoutchac catheter was then introduced by the penis, and came out again through the wound: a male silver catheter, in the common way, drew off a large quantity of urine. The same instrument was had recourse to twice or three times a day for a week, and then a caoutchac catheter being introduced with ease, a plug was inserted in its mouth, and withdrawn once in two or three hours, to discharge the urine. Thus went on the second week; after which the catheter was finally withdrawn: then the urine again issued through the wound. In a short time it began again to flow through the penis; in six weeks it all came that way; and he was able to leave Glocester the seventh week, though the wound was not entirely healed. This is the only instance which I ever knew or heard of in which ischuria vesicalis occurred during the cure of the wound made by lithotomy."

The American Mineralogical Journal, for January, February, and March 1810, *conducted by Archibald Bruce, M. D., Professor of Mineralogy in the University of the State of New-York.* Vol. I. No. I. 8vo. pp. 62. New York, 1810.

[From the Edinburgh Review, or Critical Journal, for Nov. 1810.]

At a time when the science of mineralogy is rapidly advancing in favour and consideration among the academicians of Europe, we open, with uncommon interest, a volume which announces its dawn in America. Dr. Bruce, the editor of the Journal now under consideration, thus makes known the plan and purpose of the undertaking.

"The object of this work is to collect and record such information as may serve to elucidate the mineralogy of the United States; than which, there is no part of the habitable globe which presents to the mineralogist a richer, or more extensive field for investigation.

"Of the utility of a publication of this kind, much might be said. It may, however, be sufficient to observe, that nothing has contributed more to increase and diffuse mineralogical information, than the periodical works on the Continent of Europe, particularly those in Germany and France. At the present period, when such laudable exertions are making to improve and extend the manufactures of our own country, a knowledge of the mineral productions, on which so many of the useful arts depend, and with which nature has so liberally supplied us, becomes particularly desirable.

"In order, therefore, that the design may be carried into effect, communications from those gentlemen who may have directed their attention to this interesting branch of science, are respectfully solicited, particularly such as may relate to the geology and mineralogy of particular districts; the history of mines, their products; methods of reduction, and improvements in metallurgy generally; descriptions of individual specimens, their constituent principles, localities, and uses to which they may be applied; mineral waters, their situation, analysis, and use in the arts, and in the cure of diseases, &c.

A part of the work will be appropriated to such information as may be derived from foreign and domestic journals.

"A number will be published quarterly."

Though we have infinite veneration for the privilege which exempts all periodical, and especially all quarterly publications, from the jurisdiction of reviewers, yet we hope to be excused for this one encroachment on it, in the case of a transatlantic journal, wholly dedicated to science, and of which we have nothing but what is favourable to communicate.

The first article contains the description of some minerals collected during a tour to Niagara in 1809. It is written by Dr. Mitchel, professor of natural history and botany. Among all the methods adopted for the purpose of conveying mineralogical knowledge, perhaps there is no form of composition more pleasing than that of a catalogue *raisonné*. It was this which gave such popularity to the work of Baron de Born, published at Vienna in the year 1790, one of the most useful and entertaining treatises by which mineralogy, in its infancy as a science, was illustrated. Baron de Born's enumeration, however, possessed the advantage of embracing a system for the classical arrangement of the specimens he described. Dr. Mitchell's communication to the American Journal, offers merely a descriptive catalogue of certain minerals found in the region of Niagara, and along the banks of the Mohawk river. The two first words of it bespeak a foreign idiom, characterizing, as might be expected, the Anglo-American language, in which this Journal is written. The author begins by saying, "*These pieces* were collected during a tour in the summer of 1809;" and soon afterwards, describing a specimen of black flint, he adds, "such as abounds in the Seneka *prairies*." Other examples, proving the alteration to which our language has been exposed, chiefly by the introduction of *Gallicisms*, may be noticed in the rest of the Journal; resembling expressions found in American newspapers, where, for "*a ship taken*," we read of "*a ship captivated*." So much for the style. It now remains to say a few words concerning the system patronized by the mineralogists of New-York, previous to our further examination of the contents of their Journal.

It is a fact, too well known to require illustration, that the

mineralogists—of Europe at least, are divided into two contending sects, the followers of the French, and of the German professors. The latter, with Werner at their head, are at all events the most active and stirring; and like the *Methodists* of the day, make every thing subservient to the propagation of their opinions. They preoccupy the public journals by their notions and nomenclature; tamper with the dealers; compose *gratis* catalogues for public auctions; confederate with chemists; intrigue for the appointment of their own pupils to public stations;* form societies; distribute prizes; and carry on a correspondence with every country in which mineralogy is cultivated. We must not be surprised, therefore, if we find the earliest appearance of the science in America, partially, although slightly, obscured by their influence. The general aspect is however good; and we may venture to hope, that, as the light increases, the fogs will dissipate. In our opinion, the advancement of mineralogical knowledge will be found commensurate exactly with the distance to which it leaves the unintelligible jargon of of the "*Oryctognosie*" of Freyberg. Much has been lately effected by the exertions of our own professors; and, more than all, by the wholesome exposition and pointed ridicule of the celebrated Chenevix, who, having acquired an indisputable title to pass judgment on the plan of instruction adopted by Werner, by having himself attended during his whole course of lectures, published his renunciation of the German system, and its utter inadequacy to purposes of science.

To return, therefore, to the immediate object of discussion. Dr. Mitchell's list of minerals extends only to twenty-four substances. The two first are specimens of slate and fetid limestone, from the falls of Niagara. In describing the third, a crystallization of the carbonate of lime, he says the angles of the rhomboidal crystals "are all oblique." We are at a loss to conceive any other than "oblique angles" of a rhomb. The nature of the fourth substance is not ascertained. The fifth is pronounced to be "calcareous rock, *carrying* crystals resembling

* If we are not misinformed, the valuable collection of Mr. Greville, now in the British Museum, narrowly escaped a classification into "floetz traps, transition grünstones," and a thousand other fine things, equally unknown to the collector, through the zeal of the *geognosists* who had found their way into that magnificent repository.

dog-tooth spar." The sixth, a piece of the same rock, "*charged* with iron pyrites." This is said to be very valuable, because "it contains that link in the chain of evidence which demonstrates the conversion of common limestone into gypsum." We know not "the chain of evidence" to which allusion is here made; but almost all gypsous deposits, where crystallization has not taken place, exhibit this appearance; being a result of the disintegration of preexistent strata, and therefore often containing the broken remains of those animal impressions which characterize secondary limestone. The fifteenth substance described, must be peculiarly interesting to *Caledonian* students. It is "native *sulphur*, from the Clifton Springs;"— "wagon loads of which precious mineral," Dr. Mitchell assures us, "lie on the surface of the ground!" And a little further on, he bears new and grateful testimony to the salubrious effects of that invaluable substance, both upon vegetable and animal bodies. "The moss in the neighbourhood of the "fountains is completely encrusted with it. The water of these "copious springs lets fall the *brimstone* freely on every thing "it touches; but plants *are not at all injured by it*. They appear "to vegetate *perfectly well*, and without sustaining the smallest "inconvenience. Various *animals live in it*. Horses and cattle "drink it freely, like common water."——The other substances do not require any notice; excepting the last; which is described as "an uncommon association of amianthus, graphite, and quartz."

The second article is by Colonel Gibbs. It contains observations on "the Iron-Works of Franconia, in New-Hampshire," a mountainous tract of country, eight miles east of Connecticut river. The ore is the *fer oxidulé* of Haüy. It is similar to that which is found in Sweden; and also occurs, similarly associated, in "gneiss, alternately with granite and primitive greenstone."

The third article is from the pen of Dr. W. Meade, "A Description and Analysis of an Ore of Lead, from Louisiana." This mineral resembes metallic lead in color and lustre. Its brilliancy is said to be greater than the generality of those ores which bear the name of galena; yet, as its chemical analysis proved it to be nearly a pure sulphuret, and both its crystallization

and fracture exhibited the cubic form, the difference can hardly be sufficient to separate it from those varieties. Le Sage formerly described an appearance of galena which was almost malleable. The ore analyzed by Dr. Meade, notwithstanding its resemblance to metallic lead, is very brittle; and its specific gravity 7.50.

The fourth article contains "A Geological Account of Dutchess County in New-York," by Dr. Samuel Akerly, who notices the improvement which has taken place in agriculture, of late years, owing to the introduction of *gypsum* as a manure. He decribes the mountains on the east side of the Hudson river, as consisting of masses of *granite*, occupying a region of three hundred square miles. Very few of these mountains exceed a thousand feet in elevation above the surface of the river. They abound in ores, which are said to present themselves on the very surface of the earth. Iron is the principal metal which has been extracted and worked; but tin is also noticed. To the north of this mountainous district, occur slate and limestone; neither of which substances exhibit vegetable or animal impressions; and are therefore described as of primary formation. Dr. Akerly's communication concludes with some ingenious observations upon the mineral waters in that part of America, in which he detected the presence of carbonic acid gas—of sulphuretted hydrogen—of lime—and of sulphuric acid.

"A Chemical Examination of Heavy Spar, from New-Jersey," by Mr. George Chilnton, succeeds the observations of Dr. Akerly. The mineral was found on a branch of the Delaware, by some farmers, who mistook it for gypsum; and used it as manure. It was not found, however, to possess any remarkable fertilizing quality.

The sixth article of the journal again introduces Colonel Gibbs, with "a Mineralogical notice respecting the *West River Mountain*"—Connecticut river. This mountain having been announced in the American Geography, and in the Annals of the American Academy, as volcanic, he had the curiosity to visit it. Instead of the crater of a volcano, however, he was sorely disappointed to find only the shaft of an old iron mine; whence hæmatite iron ore had been extracted, and exhibited

as lava! The mountain itself is of granite, and gneiss. To this succeed the observations of Mr. John Griscom upon "a mineral water, from Lichfield, in the state of New-York," written with great diffidence, but evincing, in a very eminent degree, the chemical knowledge requisite in the investigation. The Lichfield springs have long been resorted to by the neighbouring people, as extremely salutary in many cases of cutaneous disorder. According to Mr. Griscom, their principal saline ingredient is sulphate of lime. This substance, with a considerable quantity of hydrosulphuret, and a small portion of carbonate, may be considered as constituting the chief soluble matters in those waters, independently of their gaseous contents, a portion of which is sulphuretted hydrogen.

The next article will perhaps be considered as more interesting than any other. It relates to the discovery of native magnesia in New Jersey; and is written by the editor of the journal, Professor Bruce; who has also transmitted specimens of the mineral to this country. It is found in a very advanced state of crystallization; and, externally viewed, somewhat resembles the talc of Mount St. Gothard; but with more of the sparry lustre which characterises apophyllite. It consists wholly of magnesia combined only with its water of crystallization, in the proportion of seven to three. As it is perhaps the only mineral substance known which can be considered exhibiting magnesia, in a state of perfect purity, the observations concerning it, and its description, shall be given in the Professor's own words.

"Although Magnesia enters into the composition of many mineral substances, yet its existence in the mineral kingdom, in an uncombined state, has, till within these few years past, been unknown.

"Brogniart, in his mineralogy, has decribed several minerals under the name of Magnesite (a term he has introduced as expressive of the large quantity of magnesia they contain,) in some of which, magnesia appears to exist in a pure state. Of those which approach nearest to native magnesia, are the magnesites of Piedmont, as described by Giobert in the Journal des Mines, particularly the variety from Castella Monte. Although this mineral, as analysed by Guyton de Morveau, afforded a large proportion of carbonic acid, yet it appears, from the asser-

tion of Giobert, that, when first taken from the quarry, it contains no carbonic acid, but that it absorbs it after being two or three weeks exposed to the atmosphere.

"The magnesite from Boudissero, which is near Castella Monte, contains, in the hundred parts, according to Giobert, 68 magnesia, 12 carbonic acid, 15 silex, 2 sulphate of lime, and 3 of water. This, like the magnesite from Castella Monte, he thinks, obtains its carbonic acid from the atmosphere.

"The magnesites from Vallecas in Spain, and Salinelle in France, contain no carbonic acid: they, however, all have a large proportion of silex. That of Salinelle, according to Vauquelin, contains 55 parts in the hundred.

"Brochant mentions as native magnesia, a substance found at Robschuts, in Moravia, by the late Dr. Mitchell of Dublin, which according to Dr. Mitchell's analysis, and that of Professor Lampadius, contains nearly equal parts of magnesia and carbonic acid. Why this mineral is described as Native Magnesia, I am at a loss to determine; unless, like those of Giobert when first discovered, it contained no carbonic acid, but absorbed it after exposure to the atmosphere.

"At Hoboken, in New Jersey, on the estate of Mr. John Stevens, is found a mineral, which presents the following characters.

"Colour, white, passing into greenish white.—Lustre, pearly.—Structure, foliated; the folia or leaves frequently having a radiated position.—The folia, when separate, transparent; in the mass, semitransparent; the surface, after exposure to the weather, becoming opake.—Somewhat elastic.—Adheres slightly to the tongue.—Soft.—Powder, pure white.—Specific gravity, 2, 13.—Before the blow-pipe, becomes opake and friable, and loses weight.—Soluble in the sulphuric, nitric, and muriatic acids.—This mineral occurs in veins from a few lines to two inches thick, traversing serpentine in every direction." p. 26—28.

Then follow the Analytical Experiments by which the Professor ascertained the nature of the substance under his examination; for which, want of room compels us to refer to the journal.

The ninth and tenth articles contain mineralogical notices respecting "Phosphates of lime and lead," from Pennsylvania

—"Melanite," from the same country—and "Amber" from New Jersey; the first by Mr. Godon; and the second by Mr. Charles Wister. The apatit accompanies beril-emerald; the phosphate of lead is found in veins of pyritous copper. Melanite, according to Mr. Wister, occurs in the district of Germantown, six miles northwest of Philadelphia, in a matrix of "gneiss reposing on granite." Its crystals are of a velvet black color, varying from the size of a pin-head, to one inch in diameter. "Through the granite of Germantown, crystals of tour-"maline, mica, phosphate of lime, and beril, are universally disseminated; but melanite has been found only in one place." Amber is found *in grains* upon *wood coal;* and, from the description given by Mr. Wister, its appearances resemble specimens we have seen in Copenhagen, said to have been brought from Greenland.

The last article relates to American fluates of lime, which are said to be rare in the country. This also is from the pen of the editor. Professor Bruce describes three different appearances presented by the mineral from New Jersey, from Connecticut, and from New Hampshire. The rest of the journal, (about half the first Number of Vol. I.) is appropriated to the review of mineralogical publications; to intelligence received from other parts of America, and from England; and concludes with an Appendix, extracted from the Transactions of the Royal Society of London, concerning the identity of Columbium and Tantalium, as announced by Dr. Wollaston, secretary to the Society. The review notices a very interesting paper, published in the sixth volume of the Transactions of the American Philosophical Society, respecting the discovery of Palladium in a state of combination with gold; by Joseph Cloud, an officer in the mint of the United States. According to Mr. Cloud's experiments, palladium possesses a degree of hardness, nearly equal to wrought iron. It is malleable and very ductile; forming ductile alloys with gold, silver, and platinum; and is characterized by properties which entitle it to a place among the noble metals. Mr. Cloud has, moreover, been extensively engaged in experiments upon rhodium, which he has obtained in great purity from the ore of platinum.

We shall here close our analysis of this curious publication;

no part of which can be deemed unworthy the attention of the public. To the mere mineralogist, it is peculiarly gratifying; because it opens a new source of instruction, and a field of research, whose limits are incalculable. But science is still so new in America, that all who take any concern in its general progress, must greet the approach of such a stranger, even though they feel no peculiar interest in the errand on which he has come. His speech and manners,—even the very dress that he wears, naturally excite our curiosity, and enable us to draw inferences as to the condition of his country. A number of little circumstances, of which the editor was most probably himself unconscious, became, in our eyes, characteristic and important. We are struck, for instance, by the neatness and accuracy of the American typography; and the taste displayed in the plan and form of the work;—nay, even the advertisements printed upon its cover, afford us some insight as to the peculiar direction in which literature is destined to make its progress among our transatlantic brethren. In this point of view, we cannot avoid noticing *the air of business* which seems to play about every thing American.—Chemistry, in which they seem to have made greater advances than in mineralogy, always appears among them in connexion with some useful and gainful occupation. They have not yet found leisure to pursue it as a science of amusement. It is therefore studied only with a view to improvements in the arts of preparing cements and manures, dyeing, bleaching, distilling, purifying infected air, tanning and currying leather. These are the topics to which the chemical books, published or imported, principally refer. Other works advertised, are chiefly medical and chirurgical journals. Then the wholesale commerce carried on in these articles,—and their booksellers' shops passing under the name of "*Book Stores*," all conspire to present us with new and peculiar views of the infancy of western science. In many of its departments this business-like accuracy and method may be found very serviceable—and in none more than in that which is the subject of the work now before us. Its execution, we think, upon the whole, very respectable; and look forward, with an eye of expectation, as well as curiosity, to its promised continuation.

An Analysis of a Paper on the Phosphorescence of different Bodies, by Mr. Dessaignes, read before the National Institute in 1810.

[Translated for the Eclectic Repertory.]

THE class of natural philosophy and chemistry offered a prize for the examination of the circumstances and causes of phosphorescence in different bodies, that is to say the luminous appearance exhibited by certain bodies, whether spontaneously or excited by friction, or slight degrees of heat, or any other means distinct from combustion.

This prize was obtained by Mr. Dessaignes, principal of the college at Vendome; and the work to which the prize was adjudged at the public meeting last year, has been followed by similar experiments, which have greatly extended the facts.

This philosopher defines phosphorescence "an appearance of light durable or fugitive, unaccompanied by sensible heat, and not followed by any alteration in unorganized bodies," and he classes the phenomena under four heads, according to the occasional causes. 1st. Phosphorescence from elevation of temperature. 2d. Phosphorescence from insolation. 3d. Phosphorescence from collision. 4th. Spontaneous phosphorescence.

All bodies phosphorescent from elevation of temperature, powdered and thrown on a heated stand, become luminous whatever may be the conducting power of the stand for caloric, and the intensity of the light is in the direct ratio of the degree of heat, but the duration of the phosphorescence is always in the inverse ratio of this temperature. The last portions of light are more forcibly retained than the first, and there is in this respect great diversity in different bodies. Vitreous bodies part with their phosphorescence very slowly; while the metals, their phosphorescent oxides, and the metallic salts lose it very quickly. No degree of heat can remove the phosphorescence from lime, barytes, strontian slightly slacked, magnesia, alumine and silex. Under certain circumstances, in a moist atmosphere for instance, some of these bodies will resume their phosphorescence, whilst others will not.

This phosphorescence assumes different aspects, and like the

solar light is decomposed by the prism: it escapes in gentle emanations from some bodies, from others in scintillations; the color is blue, but generally dusky from bodies containing iron. Its purity can however be restored by depriving them of the metal which had altered its color.

In general it appeared to Mr. Dessaignes, that the most phosphorescent bodies are those which contain in their composition principles that must have passed from the gaesous or liquid state to that of a solid.

It was of importance to determine if this phosphorescence from elevation of temperature was owing to combustion. With this view Mr. Dessaignes made his experiments in atmospheric air, oxigen, and the torricellian vacuum, without observing any difference in the intensity of light from unorganized bodies; but the light from organized bodies was increased in oxigen; whence the author infers that in these bodies the phosphorescence is partly occasioned by combustion.

But an elevation of temperature does not render all bodies luminous, and those which acquire phosphorescence from this cause, lose the property under certain circumstances. On what cause then does inphosphorescence depend?

Such is the question proposed by Mr. Dessaignes, for the solution of which he repeated his experiments, introducing such circumstances as favoured his views. His researches have led to the following results. 1. That the products from fire are not luminous unless the bodies have passed from an earthy state to a state of vitrefaction. 2. Bodies having the water of crystallization in excess afford no light. 3. Bodies capable of being softened by heat likewise afford no light. In this condition are the salts with excess of acid, the boracic salts excepted, they not being melted at the degree of heat employed in these experiments. 4. Bodies and particularly salts which are volatilized, or decomposed at this degree of heat are not phosphorescent. 5. And lastly, bodies mixed with a large quantity of the metallic oxides are likewise completely obscure.

Nevertheless most of these bodies may become luminous on being moistened, provided they possess the property of combining with water, and of rendering it to a certain degree

solid. In short, this property may be restored to those bodies that had lost it, on their condition being changed.

Mr. Dessaignes concludes from his experiments, of which we have only been able to indicate the result, that the phosphorescence produced by an elevation of temperature, is owing to a particular fluid driven by the caloric from the bodies between whose particles it exists; and he considers this fluid to be the same as electricity. He entertains this hypothesis, because the circumstances which favour or destroy the accumulation of the electric fluid in like manner favour or absolutely destroy the accumulation of the phosphoric fluid in the same bodies, and that because electricity may be directly accumulated in bodies so as to render them luminous.

It had long been known that the exposure of certain bodies to light rendered them phosphorescent. Dufay and Beccaria had made experiments on the phenomena of this kind, and from the experiments of the latter it was inferred that the phosphorescence of bodies exposed to light arose from the disengagement of the light which had been introduced by a species of absorption. But the experiment on which this hypothesis is framed has been ascertained by Mr. Dessaignes not to be correct: the phosphorus which he subjected to the different prismatic rays always afforded the same kind of light. Moreover the phosphorescence produced by insolation, far from being an emanation of rays, is only an oscillation; for however frequent the insolations, the phosphorescence is not increased, and the covering the phosphorescent body with smoke will render it obscure. Light as well as heat does not render all bodies phosphorescent, and those which are, are not so in the same degree. The Canton phosphorus becomes phosphorescent by the light of the moon, while the hyalin quartz shines only by the immediate light of the sun. In general all fluid bodies are insensible by this kind of excitation, which is the case with charcoal, the carburet of iron and other metals, most of the sulphurets, the metallic oxides in the dry way, and in general all bodies which like the foregoing are conductors of electricity; but the edio-electric may become phosphorescent by means of a strong light. It ought to be observed that in

respect to the phosphorescence all bodies exhibited the same effects with electricity as with light.

The light produced by insolation and by heat have the same color, and both are equally affected by the metallic oxides.

Bodies the most luminous by insolation cannot be rendered so when heated; but they become phosphorescent in proportion as they cool, and some bodies which have lost the power of shining by an elevation of temperature become luminous by insolation, which Mr. Dessaignes attributes to the water retained in these bodies; for water has unquestionably great influence in these phenomena, as Mr. Dessaignes has in several places remarked.

The light produced by many bodies known by the name of phosphorus has been ascribed generally to combustion. Mr. Dessaignes being desirous of examining this hypothesis, subjected these bodies to experiments which according to him evidently prove that they owe their light to the same cause, to wit, a species of electricity; for Mr. Dessaignes considers the light produced by irradiation and electrising the same as that afforded by an elevation of temperature; only that in the two first instances the light experiences a kind of vibration, while in the last it is really expelled.

Mr. Dessaignes made the phosphorescence by collision the subject of several memoirs. From his experiments the following general and very remarkable law is established; that all bodies whether solid, fluid or gaseous afford light by compression. But this light is not so abundant when the body has been rendered phosphorescent by heat; and however frequent and strong the compression, they cannot be entirely deprived of their phosphorescent property. Mr. Dessaignes thinks that this light depends on a different cause from that produced by heat. "It appears to depend," says he, "upon a very elastic "fluid closely united to all the elements of gravitating matter. "This fluid, the primary source of expansion, is pressed out "in proportion as the constituent particles of matter approach "each other, so that it is farther from the limits of compression "in gases than in vitreous bodies; and in the latter a less effort "is required to excite the oscillations, &c."

Mr. Dessaignes distinguishes two kinds of spontaneous phosphorescence, the one fugitive;—the other permanent. Among the former may be classed those produced by the union of a certain portion of water and quicklime, and among the latter rotten wood, and other bodies in a state of putrefaction. The latter particularly engaged the attention of Mr. Dessaignes. His observations were made on animal substances, fresh water and sea-fish, on vegetable matter, and the different kinds of wood. These substances individually presented distinctive characters, but from their united phenomena it appears that their phosphorescence is a species of combustion in which water and carbonic acid are formed; all the parts constituting muscle or wood do not contribute to the production of light in these bodies; the woody and muscular fibre undergo no essential change, and the phosphorescence of these bodies is owing in wood to the glutinous principle and in muscle to the gelatinous principle uniting their respective fibres.

From the numerous facts collected respecting spontaneous phosphorescence, Mr. Dessaignes endeavours to explain the phosphorescence of the sea, which he thinks may be owing to two causes. 1. The presence of animalculæ, phosphorescent by the emanation of luminous matter secreted by the animalculæ. 2. By the mere presence of matter dissolved or mixed in water and proceeding from these animals, or fish and the mollusci, &c.

Since the publication of his first work, Mr. Dessaignes has extended his inquiries; and has attempted by numerous experiments to determine the influence of points on the phosphorescence, whether from elevation of temperature, or from insolation; and he not only ascertained that the points have the same effect on the phosphoric fluid as on the electric; but moreover that natural bodies differing only in the aggregation of their particles, may admit of infinite variety as regards their phosphorescent property, &c.

An additional Account of the lythontriptic Power of the Muriatic Acid, with an Analysis of the calculous Matter discharged in one Case, during its Use; also an account of the lythontriptic Power of the nitrous Acid, with an Analysis of the calculous Matter discharged in a Case during its Employment. By PETER COPLAND, Little-Bytham, near Stamford.

[From the Medical and Physical Journal for May, 1811.]

THE attention of Mr. Copland to the lithontriptic properties of the muriatic acid, was first excited by cases of its successful employment, related in the fifth and sixth volumes of the Memoirs of the Medical Society of London.

The cases here detailed afford sufficient evidence to induce a further trial of the muriatic and nitrous acids in calculus. In the first, thirty drops of the muriatic acid were taken in water three times a day; the dose was gradually increased to fifty drops, and continued till two ounces were taken, when the complaint was removed. The patient was directed to collect daily, in one vessel, all the urine which could be obtained in twenty-four hours. The clear urine was then poured off, and the sediment collected upon a paper filtre. The sediment thus collected amounted to 104 grains, of a buff coloured impalpable powder.

One hundred grains of this powder subjected to chemical analysis, was found to contain

	Grains.
Uric acid	72
Ammonia	18
Carbonate of lime . .	3
Phosphate of lime . .	5
Loss	2
	100

In the other case, (that of Sir Simon Kelly, of Colsterworth, Lincolnshire) forty drops of the diluted nitrous acid were taken in water every two hours, till a sediment appeared in the urine; and afterwards continued four times a day, while necessary. Twenty-seven ounces of the diluted acid were used. A sedi-

ment soon appeared, and by persevering with the medicine from November 12, 1805, to April 20, 1806, 600 grains of a light thick coloured powder were collected; in which, toward the conclusion, a few fragments of calculus were found, partially decomposed. One hundred grains of this sediment, subjected to analysis, gave

	Grains.
Uric acid	80
Ammonia	11
Carbonate of lime . . .	2
Phosphate of lime . . .	1
Loss	6
	100

During the treatment of these cases opium was occasionally given to mitigate pain; costiveness was prevented by mild laxatives; and when the stomach was oppressed by the frequent doses of the acid, it was relieved by taking any convenient spirit diluted water. It appears, from this gentleman's observations, that the *nitrous acid* possesses greater lithontriptic powers than the *muriatic:* and that in many cases of *lithiasis*, it always procured a discharge of sediment with the urine; and a due perseverance in its use was followed by a removal of the symptoms.

Memoirs of the Life of Thomas Beddoes, M. D. with an Analytical Account of his Writings, by John Edwards Stock, M. D. 4to. pp. 413. Murray. 1810.

[From the British Critic for June, 1811.]

In all biographical publications the reviewer must be supposed to have *two* objects before him, the subject of the history, and the biographer. We have in the present work an account of the life and writings of an eminent physician, written by a brother physician of reputation and abilities, and were we to go regularly through the book, in order to present the reader with a critical commentary on its contents, we should have

perhaps nearly as much to say of the historian, as of the person whose history he writes. We do not mean to intimate that Dr. Stock really writes about himself, but in giving an account of the writings, remarks and discoveries of his friend, he intersperses further remarks of his own, so as to render his history a kind of review of Dr. Beddoes's writings and opinions. Now it so happens that almost all the writings of Dr. Beddoes, mentioned in this volume, have already been reviewed by ourselves, nor do we see any particular occasion to alter our opinions, so that in our account of the work before us, we shall have more to do with the man than with the physician and philosopher. The lives of literary men, Dr. Stock observes, seldom supply the biographer with much diversity of incident; we are bound, however, at the same time to acknowledge, that what they may want of this popular attraction, is for the most part likely to be compensated by the importance and utility of the researches and occupations to which such persons may have devoted their time; and from the nature and character of the particular pursuits in which Dr. Beddoes engaged, we are very ready to admit, that no man's life could well have been more uninterrupted, zealously and enthusiastically applied to the advantage of his fellow creatures, than that of the learned person here preserved from oblivion. In saying this, we lay out of the question, the actual utility and consequences of his labours and researches; were they to turn out ultimately of no account at all to mankind in general, the Doctor would still deserve the fair praise due to every one who bestows his time and attention wholly upon pursuits so intimately and immediately connected with the welfare, happiness, and good of his brethren. We think it incumbent on us to say this, openly and explicitly, because having before us the testimony of very respectable persons, to the disinterestedness and purity of all his aims and intentions, we cannot regard him in any lower light, than as a most ardent philosopher, and sincere friend of man. We are the more disposed to say this, because in our former reviews of his writings and opinions, we have frequently found occasion to differ from him, and sometimes have been led to espouse the opposite side with an earnestness and warmth of feeling, called for as we then thought, by the strong terms in which Dr. B. himself was wont to assert his own opinions; not

always (undoubtedly,) with that liberality of sentiment and decorum of manners, the want of which he was forward to resent in all cases that applied to himself. He was undoubtedly an eccentric man, but eccentricity may lead to good as well as harm. It is not therefore any positive fault in itself; and it is well to have shown, as the present biographer *has* done, that the eccentricity of Dr. Beddoes always tended towards the good and happiness of man, however attainable; these were the objects he constantly had in view.

The Doctor was by descent of Welsh extraction, though born himself at Shiffnall, in Shropshire, April 15, 1760, where he received the first rudiments of his education, but was soon removed to Brood [or Brewood] in Staffordshire; he very early displayed a thirst for knowledge, and, as is frequently the case, appears to have been determined rather by accident than design, to adopt the line in which he afterwards most distinguished himself. From Brood he was removed to the free grammar-school at Bridgenorth, which he quitted again at the age of thirteen. His manners and habits at school were peculiar. He seemed early to give way to deep thought and reflection, and this, added to a natural shyness of disposition, gave him an air of reserve, which distinguished him from his young associates. In May, 1773, he was placed under the tuition of the Rev. Samuel Dickenson, rector of Blym-hill, in Staffordshire, who supplied the biographer with some particulars of character, highly creditable to him. In 1776, he was removed to Pembroke College, Oxford; here he applied himself with remarkable industry and diligence to the study of modern languages, chemistry, mineralogy, and botany. In 1781, he visited the metropolis, and studied anatomy, under Sheldon. In the course of these studies it was, that he undertook to translate and publish the works of Spallanzani, which appeared in 1784. Dr. Stock is inclined to think he was undoubtedly the person who supplied the notes to Dr. Cullen's edition of Bergman's Physical and Chemical Essays. In 1786, he edited Scheele's Chemical Essays. In 1783, he took the degree of master of arts. In 1784, he went to Edinburgh, where he distinguished himself, not only as a member, but for some time *President* of the Royal Medical and Natural History Societies;

he was highly gratified with his residence at this place. In 1786, he returned to Oxford and took his doctor's degree; in the same year he visited the continent, and became acquainted with various eminent naturalists, &c. It was shortly after his return from the continent, that he received the appointment to the Chemical Lectureship at Oxford, in which situation he certainly distinguished himself much, and was generally attended by a class, not only numerous, but particularly respectable. Mineralogy at this time appears to have occupied much of his attention; his Theory of the Earth, Dr. Stock tells us, was conformable to that of Hutton, and he was a zealous *Vulcanist.* We know this to have been the case; but we also know, that at this time he was particularly hasty in his conclusions, and would frequently acknowledge that he had been misled in the judgment he had formed of certain fossils, especially in regard to the operations of fire; and we can cite a very remarkable instance of this, which happened very soon after his *first* opinions had been publicly declared and maintained before a large audience.* At this time nothing seemed to interest him more than the account of the Two Giants' Causeways, or groups of prismatic basaltine columns, in the Venetian states, in Italy, in the LXVth Vol. of the Philosophical Transactions, communicated by Mr. Strange, long his Majesty's

* The case was simply this. The writer of this article had brought to Oxford from the summit of one of the mountains surrounding Coniston Lake, in Lancashire, some specimens which had evidently undergone the operation of fire, but which happened to abound near a *hollow* on the top of the mountain, which some *Italian* gentleman had not long before pronounced to be the crater of an extinct Volcano. Upon showing them to Dr. Beddoes, he was so persuaded of the fact, that he even summoned a particular assembly of the members of the University, by an *extraordinary notice,* before whom he delivered a long lecture on the specimens supplied, as indicative of the natural operations of fire in those parts of England. A very short time after, the writer of this article having reason to think he had changed his opinions, questioned him again about the same specimens, when he protested that they were evidently nothing better than mere flags from some old furnace, and that he had since discovered a criterion by which he could distinguish between the productions of natural and artificial fire, but this discovery, and the consequent change of his sentiments, he could not be prevailed on to announce as publicly as he had delivered his former opinions; in fact, he had engaged in new pursuits, and he concluded the University would discover the mistake without being told of it by himself.

Resident at Venice, a circumstance incidentally mentioned by himself in the work before us, and to which from an acquaintance at the very time, with both the learned persons alluded to, the writer of this article can bear ample testimony.

Many pages of the work before us are taken up with a poetical attempt of Dr. Beddoes in the style of Dr. Darwin, and some discussion is entered into, upon the merits of the latter gentleman's productions. As our opinions upon this subject are stated elsewhere, we need not enter upon it again; Dr. Stock is evidently a great admirer of Dr. D., but he admits, that the public has been much divided in regard both to the Poetry and Philosophy of the celebrated physician and bard of Derbyshire.

Dr. Beddoes' retirement from Oxford, which we believe took place in the year 1792, seems to have been accelerated by his intemperance in politics, occasioned by the remarkable circumstances of the times. Dr. Stock has taken some pains to soften and palliate every thing that was exceptionable in the Doctor's conduct, and to give a fair and candid account of the motives by which he was probably actuated.* It was in the year 1793, that the Doctor removed to Bristol, where he began that career of medical and physiological researches, experiments, and lectures, which made him so generally conspicuous, and which appear to have been continued with the most striking zeal and perseverance to the last moment of his short life, varied according to circumstances, but never wholly abandoned.

At Bristol Dr. Beddoes first formed a connexion with the family of Mr. Edgeworth, the author of the popular work on Practical Education, whose daughter he afterwards married.

* We are bound to believe that Dr. Beddoes was far from wishing ever to inculcate "more than constitutional resistance," for he positively asserts it himself in the work before us, p. 117; that he was sincere also in "deprecating all violence," and could "shudder at the idea of *confusion*," we are likewise disposed to believe, not only from his own declarations, but from what we knew of the private temper and amiableness of the man; but undoubtedly he often spoke and often *wrote* of public men and measures, in a strain, which was, to say the least of it, liable in the nature of things to produce an irritation, which might have much exceeded the bounds he would have himself prescribed: a case by no means uncommon.

In the year 1798, the two elder sons of W. H. L. Esq. M. P. (we presume Mr. Lambton, member for Durham) were placed under the care of Dr. B. in conformity, as it would seem, with the express wish of their deceased parent, to be brought up on Dr. B.'s *own plan of education*. They continued with him four years, and the learned historian remarks, that upon this occasion "he realized very happily, in his mode of instruction, many of his former *theories* on the subject of education; and the results were highly gratifying." This is certainly as much as the editor could be expected to say, and we are not inclined to dispute it, not being ourselves in the way of appreciating the effects alluded to.

In the same year, the Pneumatic Institution was opened, which is certainly particularly memorable as an era in philosophy, if for no other reason, yet for this, that it was almost the first stage on which Mr. Davy began to exhibit his extraordinary talents. We are not unwilling, however, to assign to this Institution a credit of its own. Though apparently forgotten and done with at the present moment, we confess we feel inclined to agree with Dr. Stock, that *perhaps*, "Pneumatic medicine has fallen into premature and unmerited oblivion," p. 314. Gaseous remedies appear to us undoubtedly to be capable of considerable effects, and we see no reason why they may not by judicious methods of modification and exhibition, simple or combined, be some time or other brought to bear no inconsiderable share, in the alleviation certainly, and possibly in the perfect cure of some of the worst diseases to which mankind is liable. We judge only from the effects unquestionably and undeniably produced by their exhibition; which, however incapable at present of being reduced to certain principles, and directed to certain ends, may yet, we apprehend, in time, be better understood, and more scientifically applied.

We are much inclined also to give Dr. B. great credit for his wish to disseminate certain *principles* of medical knowledge among all classes of persons. We are confident that a just conception of the true nature of the *first symptoms* of disease, would indeed tend greatly to the lessening the amount of fatal cases, and exceedingly alleviate and abridge the sufferings of the species at large. "Preventive and prophylactic medicine,"

in short, is, in our opinion, a grand desideratum, especially among the poor, whose ignorance upon this particular point exceeds any thing that can be conceived, and is, we fear, the occasion of the loss of many children. Still, however, we are aware of the amazing difficulty of teaching those who are resolved not to learn. Quacks and empirics, and village gossips, against whom Dr. B. was particularly severe, would, we are confident, long maintain their ground against the wisest and best conducted efforts of regular practitioners. We cannot forbear extracting one of the Doctor's remarks on a certain class of persons just alluded to, namely, Village Gossips, who

"Without faculties to comprehend the laws of the human system, even if they had attempted to study them, force upon their friends some infallible recipe, for any disorder with which they may be afflicted."

The Doctor observes, that

"It is particularly worthy of remark, that the interference of these volunteer practitioners is almost uniformly confined to the medical province alone: were any one of them to produce her case of instruments, it is doubted whether even the politeness due to the sex would induce submission to any surgical operation, though it were only proposed to open a vein; and yet," adds the Doctor, "the tools of the physician are not less keen than those of the surgeon: but a simple reason will explain the difference; their operation is secret*." P. 198.

We are confident Dr. Beddoes had a just view of many real evils subsisting in the world, though perhaps he might see others in too strong a light, and concerning some, be altogether mistaken. That in regard to *all* errors and abuses, he was generally too sanguine about the practicability of their speedy removal, and too hasty in applying his remedies, and certainly by no means discreet in his mode of employing them, we most fully admit; but we are yet willing to give him credit, for projecting many things, which under other circumstances, and

* From a remark of the Doctor's, however, to be found in the appendix, it appears, that he thought quackery not upon the increase, but that "in proportion to the population of the country, medicine is now less frequently administered by unprofessional hands, than at any former period."

even in other hands, might possibly, with great advantage to the world in general, be carried into effect, and probably one time or other will be realized.

It is very melancholy, but highly instructive, to read the account of Dr. Beddoes' end. Nothing can be more calculated to repress all extravagant prospects of sublunary happiness, or that confidence which we are all too apt to place in the future events of life. With a mind as active as ever, perhaps more so than at any other period of his life, with an imagination teeming with projects and speculations, upon points the most important, happy in his family, comfortable, perhaps even affluent in his circumstances, and so little advanced in the career of life, as fairly to admit the hope of many years to come, he is suddenly arrested by the hand of death; and notwithstanding all his own skill, and the care and advice and closest attention of many eminent medical friends, hurried to the grave, and removed from a stage on which no man was calculated to act a busier part. Though himself a practitioner, he seems, as is not uncommon, to have entertained wrong notions of his malady, and probably at last fell a victim to a complaint, least suspected by himself, though not so by the friends who knew him. He judged his hepatic system to be out of order, but the complaint lay in his chest, his respiration had always been difficult, and sensibly so to his acquaintance. Dr. Beddoes died before he had completed his 49th year.

The character which Dr. Stock his given of him we have read with pleasure; we verily believe it to be just; privately we always entertained a regard and high respect for him; publicly we often differed from him, and our opinion was no less fixed and determined. We ever accounted him capable of great things, but too hurried, too sanguine, too unconscious of the lapse of time, and too little aware of the want of opportunity for any one man to accomplish any very numerous ends, either of invention or reformation. To effect all that he wished to do, he ought to have been able to calculate upon a residence here of at least a thousand years, but this being evidently impracticable, it would have been better if he had not attempted too much, but had fixed his whole thoughts, and applied all his time to some one design. It was, however, as possible to stay

the lightning, or regulate the winds as his movements; they were too rapid to take any steady direction; and he was easily turned aside from any pursuit by new objects of curiosity or inquiry.* As he was not above changing his opinions upon conviction, it appears much to be wished, that so well meaning, and so very ingenious, a man, could have lived much longer, but his premature death is among the events, which in our present state, we must not presume to fathom or understand—Providence sees not as we see.

Dr. Stock has enlivened his book by many pleasant traits of character, displayed principally in the intercourse of his friend with other eminent persons, his correspondence, and his occasional remarks.

The following account of his reception of a learned foreigner, is so perfectly characteristic of the man, that we cannot possibly omit to transcribe it; though already selected by other reviewers.

"In the year 1803, Doctor Joseph Frank, of Vienna, having visited England, for the purpose of examining into the state of our prisons, and charitable institutions for the sick, called upon Dr. Beddoes, at Clifton. The account of the interview we have from himself, and it is briefly as follows. 'All his acquaint-

* As Dr. Stock has particularly noticed this feature in his character, we shall not scruple to relate the following circumstance, to show how rapidly and *inconsiderately* he passed from one pursuit to another. In the summer of 1792, Dr. B. applied to the writer of this article, then at Oxford, to find him some young person, capable of translating French, and acquainted enough with mineralogy and chemistry, to undertake an English edition of *Dolomieu's* Travels to the *Lipari* islands. Instead of recommending any third person, the writer of this article, being perfectly at leisure, undertook the translation. Dr. B. was much pleased with the offer, undertook to spare no efforts to make it an important work; and accordingly wrote to *Dolomieu* then at *Paris*, to apprise him of the undertaking. He engaged a most ingenious German, then at Oxford, to make drawings of certain fossils; he engaged Messrs. Cadell to print the work, and take the charge of the engravings, and the whole for which he made *himself* responsible was, to *write a preface*. The event, however, really was, that though *M. Dolomieu* much espoused the work, and sent over many drawings of fossils, from the king's library at Paris; though the German draftsman copied many of them most beautifully, though Messrs. Cadell stood to their agreement, and the translation was fully completed and put into his hands, the whole came to nothing: his thoughts had taken a different turn, and he could not be induced to take a step further in the business.

ance,' says Dr. F. 'had told me beforehand, that I should find in Dr. Beddoes, a man, whose *premièr abord* was rather repulsive. On entering his house, I gave the servant my introductory letters, that his master might be somewhat prepared, and not taken by surprise. After waiting about a quarter of an hour, Dr. Beddoes appeared with several books under his arm. The first words that he addressed to me, were, 'which Dr. Frank are you? for there are a great many of you.' Before I could answer him, he laid before me, in a row, several books, all written by Franks, constantly asking as he turned them over, 'Is that you? Is that you?' The first that met my eye, was a Materia Medica, by Solomon Frank, I protested against this being mine. Then followed some of the works which I had written in elucidation of the Brunonian system. Having now recognized me, Brown became the first topic of our conversation, &c.'

In an appendix we have two curious papers, which were read by the Doctor before the Natural History society of Edinburgh, the one on the sexual system of vegetables, the other on the scale of being. In which, as Dr. Stock remarks, "he maintains the negative of both these questions with considerable ingenuity." In the latter, there is a good deal of humorous satire, very entertaining certainly, but rather unsuitable to the dignity and gravity of a Philosophical society. The following specimen will amuse the reader.

"To shew the difficulty of adjusting the claims of different beings to certain stations, it will be proper to consider a few instances. I shall suppose that man has taken his place, and that a competition arises among the other animals for the honour of standing next the Lord of creation. The ape will urge his form; the half-reasoning elephant, his superior intelligence; the beaver his mechanic arts, his dependence on the cooperating powers of many associated individuals for his well-being; and may not the pertness of the parrot produce in her favour, the power of uttering articulate sounds? nay may not the abject bradypus, which may be said rather to grow than to live, and in sagacity is lower than the insects, put in a still more powerful claim? If native helplessness, if paucity and weakness of instincts be among the characteristics of man, then will an im-

partial judge hesitate, before he dismisses this plea as inadequate; and if he, moreover, takes into consideration the circumstances of pectoral mammæ, and no very distant similitude of habit in other respects, he must either invite this seeming outcast of nature to a seat by his side, or pronounce the contest doubtful.

"It would be no less difficult to find the next link to the bradypus: for it is allied to the Pecora by the property of rumination; to the birds in having a common cloaca; to the amphibious animals, by tenacity of life, and faintness of vital actions; to the myrmecophagus, in having strong and reflected nails, and in wanting dentes incisores. From the myrmecophagus, the manis differs only in having scales, instead of which the Darypus is provided with a shell, by means of which this genus is allied to the Testudines. I mention these last marks of resemblance, merely to show what kind of a chain might be constructed if equal or stronger marks of resemblance were kept out of view. The following would be a few of the links:

Homo sapiens,
Bradypus,
Myrmecophagus,
Manis,
Darypus,
Testudo.

Whence it appears that we might get from an alderman to a turtle at five steps."

In the appendix also are some very amusing extracts from the Doctor's common-place book, which are well calculated to show the course and turn of his thoughts, and the many experiments he had in view, some of the latter, however, though connected with very important researches, will scarcely be read without a smile, such for instance, as follows:

"Memorandum.—To try *this*.—To *opiate* a number of frogs near an air-thermometer. Also to *digitalise* them. *Phosphorus*.—To eat a little; bleed; smell and try the air in the dark if luminous."

The following articles pleased us as well as any.

"*Nature*—It is a vulgar error to say such an invalid must be left to nature; the thing is nearly impossible. Nature has

no temple in which the sick can be deposited. Medicines made up at an apothecary's may be discarded, but some sustenance must be administered, and some plan of management followed, and the circumstances of these may be far from indifferent. What they call nature is all *art; art*-fed, *art*-dressed *victuals*, as artificial as medicines."

"*Names*—The force of genius preserves a writer against certain faults of taste. Shakspeare calls scarce any of his characters by adjectives, expressive of the character he means to paint, except Shallow and Slender. The vulgar author of the Pilgrim's Progress, vulgarly labels all his. It is a miserable shift to help out deficiency in dramatic drawing and colouring. It should be left to the reader to find out the proper epithet. The name and nature of different members of a family are put sadly at cross purposes. If the hypocrite hero of the School for Scandal, is to be baptized Joseph Surface, his brother ought to have stood in the *dramatis personæ*, as Charles Bottom."

We must now take our leave of Dr. Beddoes, and we do it with unfeigned regret, lamenting his early end on many accounts, but especially because we think age might have corrected some of those blemishes, or rather brilliant eccentricities in his character, which prevented his doing justice, even to his own designs, and his own powers. Had he been less impetuous, less sanguine, and more capable of fixing and concentrating his views, he might have accomplished much more good, and left the world much more benefited by his extraordinary labours, and indefatigable diligence. To the work before us is prefixed an engraving of the Doctor, by Warren, from a painting by Bird, which we think one of the happiest resemblances we ever remember to have seen.

ORIGINAL REVIEW.

An Inaugural Dissertation on Mercury, embracing its Medical History, Curative Action and Abuse in certain Diseases. By John W. Francis, A. B. Vice-president of the Medical and Surgical Society of the University of New York. New York, 1811.

Dr. Francis informs us that in this essay, he has made an attempt to exhibit a concise account of the natural and medical history of mercury; to found the theory of its curative action upon principles believed to be less liable to objection than those upon which it has been hitherto explained: and to offer some observations on the mercurial practice, now more generally employed by the physicians of this country, in the treatment of certain diseases.

The natural and medical history of mercury is very briefly treated, for the purpose of proceeding to the more immediate objects of the writer—the consideration of the curative action of the remedy and its abuse as an article of the materia medica.

The writer, nevertheless, in his natural and medical history of this powerful medicine, has displayed proofs of considerable research, and by his copious and judicious references, enables the reader to consult the sources from whence he has drawn his knowledge.

He closes this section with the notice of its introduction into extensive use in the diseases of warm climates; more particularly in the malignant fevers of the tropics, by Dr. Chisholm, and the practice subsequently adopted by the North American physicians.

He next attempts the explanation "of the curative action of mercury"—previously to which, he ingeniously controverts the opinions of Boerhaave, Hunter and Bell, respecting its modus operandi, for which we must refer our reader to the work itself. Our author's opinion would appear to be that the curative action of mercury depends upon its very general and stimulating operation in exciting the *excretories of the whole*

*system.** For he observes that the same salutary effects will not be produced when its action is not thus extensive; as when its operation is confined to the salivary glands, or when it exerts its influence on the intestinal canal alone. That its medical virtues, continues he, depend principally upon its operation on the exhalent vessels of the skin, at the same time that it operates upon the other emunctories of the body, is still further evinced by the superior benefit which is derived from certain preparations of this metal, which are known to operate almost exclusively in producing diaphoresis: as the combinations of it with the marine acid in the form of the corrosive sublimate. Proofs of a like nature, continues our author, may be drawn from the action of those remedies, which are often united with mercury for the more advantageous treatment of the venereal disease, as antimony, guiacum, sarsaparilla, opium, &c. Again, experience has uniformly shown, that if the action of mercury in causing diaphoresis, be aided by a proper regard to temperature, food and regimen, its salutary effects are greatly increased: and if its operation be prevented or suppressed by irregularities in diet or from exposure to cold, they are greatly diminished.

He thinks that inasmuch as mercury produces a quickened action of the bloodvessels, and a consequent increase of all the secretions, it may justly be considered as a most powerful alterative; and as it possesses in a special degree the property of exciting the excretory vessels of the skin, intestinal canal and kidneys, it prevents that peculiar operation from taking place upon the absorption of a specific matter into the system, which is termed by several practical writers the *assimilating process.*

The most copious and interesting section of the essay now under analysis, is that which treats "of the effects of mercury in its native state and of its abuse in certain diseases."

Our author here finds that its action upon those individuals who employ it in the various mechanical arts, or who are

* And for this opinion, he acknowledges himself indebted to his preceptor Dr. Hossack.

themselves engaged in the working of mercurial mines, is at times peculiarly deleterious.

In support of this statement, he quotes among others the celebrated names of Agricola, Fernelius, Acosta, Hildanus, Ramazini, Hoffman, Jussieu, Mead, Sauvage and Gmelin; and finds but one writer of eminence who controverts this opinion—in the indefatigable traveller Humboldt, who attributes the pernicious consequences which ensue from the working of mines of that metal, to the excessive labour the individuals engaged in this employment undergo.

Non nostrûm est tantas componere lites.

We shall only venture to observe, that the weight of the testimony adduced above, aided by the high authority of Boyle, would seem to preponderate against the simple assertion of the illustrious baron.

Dr. Francis next treats of mercury as an article of the materia medica, and observes that it contains not an article in the employment of which, there are more circumstances deserving consideration, than those connected with or attendant upon the use of mercury. This remark with nearly equal force, he finds to apply to all the various forms in which this powerful remedy is at the present day administered; and he who ventures, continues he, to depend upon the successful operation of this medicine, without a due regard to the age, sex and particular constitution of the patient; the diagnostic symptoms and stage of the disease; the previous treatment that has been pursued; climate and temperature; will not only subject himself to frequent disappointments and hazard the safety of his patient, but most deservedly become the object of severe crimination.

A general view is then offered of the action of mercury when taken into the system in considerable quantity, either by the mouth or by absorption from the surface, and he finds that it acts with peculiar force upon the stomach, inducing indigestion, loss of appetite, nausea and vomiting; the functions of the chylopoietic viscera becoming greatly disturbed, especially those of the liver; and from its very powerful effects upon the digestive organs, the whole intestinal canal soon participates

in the disorder. It exerts a no less powerful influence upon all the secretory and excretory vessels of the body; as in those of the mouth and fauces, trachea, bronchiæ and lungs; and, not unlike the preparations of antimony, paralizes the action of the minute and distant vessels, especially those of the lymphatics. From this most general operation a languor of the whole system is produced, the constitution at large becomes affected, and symptoms highly indicative of general derangement and nervous irritability ensue. These effects, however, are much more readily induced in particular individuals or habits of body than in others; and an intimate relation exists between the operation of this medicine, and the age, sex, constitution of the patient, temperature, &c.

Some of the more ordinary ill effects attendant on the frequent and indiscriminate use of mercury, are then particularly detailed, as well as the numerous disorders which it induces of the nervous system and of the intellectual functions, which the limits of this department of our work do not permit us to specify.

Dr. F. next enters upon the consideration of what he terms "the present fashionable employment of this remedy in the treatment of particular diseases." His remarks, he says, are "necessarily confined more especially to the practice of physicians of our own country."

And first of some of the diseases belonging to the class denominated febrile.

To the bold and vigorous practice adopted by the celebrated Dr. Chisholm, says our author, in the cure of the malignant or pestilential, or yellow fever, which prevailed at Grenada and at other of the West India islands, in 1793, may be attributed the present general employment of mercury in the treatment of all the various forms of intermittent, remittent and typhus fever in the United States—and to Dr. Chisholm exclusively belongs the credit of being the first who adopted the novel practice of exhibiting mercury to the great extent it is now employed in the disease first alluded to—for during the existence of the same epidemic in subsequent years, sometimes no less than 800 and at other times upwards of 1000 grains of calomel were employed. Indeed a case where mer-

cury was used to a much larger extent is related from Chisholm's Essay on the Malignant and Pestilential Fever.

Dr. F. next takes a view of the introduction of mercury in the treatment of this *epidemic*, when appearing in different parts of the United States, and briefly notices the difference of opinion that subsisted among medical men with respect to its beneficial effects in the disease.

The utility of the free employment of mercury in the treatment of the yellow fever of the United States, may be well doubted, says he, chiefly for the following reasons:

First. The climate of this country is singularly unfavourable to the salutary operation of this medicine. The frequent and uncertain transitions of the weather from heat to cold, from dryness to humidity, while they are often the cause of sudden and violent diseases, which generally assume the inflammatory character, they give the body a certain morbid predisposition that renders it unable to withstand a mercurial course, which on the nature of a more southern and temperate atmosphere would at least prove not injurious, and probably successful.

Secondly. The morbid appearances as they are exhibited upon the dissection of individuals who have died of the yellow fever in different parts of the United States, during its prevalence in different years, furnish another argument against the extensive employment of mercury in this disease. As it is universally admitted that this remedy is a powerful deobstruent, and indicated in all cases of local congestion, particularly of the glandular system; from a supposition that this part of the body was especially affected, implicit recourse was had to mercury. But while the liver appears to be the most diseased organ of those who die of yellow fever in the West Indies, this important viscus seems to be in a remarkable degree exempt from derangement in those who have died of the same disease in this country. [Exceptions, however, he acknowledges, occurred in the yellow fever of Boston in 1798.]

As our author supposes that glandular obstructions seldom take place in the yellow fever, he consequently reprobates the producing of salivation to remove an imaginary cause, and afterwards states upon the authority of Dr. Hossack and Dr.

Osborn, that even in cases where salivation was effected, a fatal termination of the disease followed. He asserts that the use of calomel in the malignant epidemic must necessarily aggravate the gastric and intestinal affections, so frequently distressing in this complaint, and quotes in proof of this assertion, the experience of Doctors Bard, Hossack, Currie, Chatard and Wheaton.

It may perhaps occur to some of our readers, that as Dr. Francis seems inclined to banish mercury from the list of remedies in yellow fever, it behoves him to suggest some preferable plan of treating the disease, and he accordingly does not hesitate to declare that what may be denominated the *sudorific* plan appears to be by far the most successful. "After the necessary evacuation from the stomach and bowels, and in some instances from the bloodvessels by the lancet, says he, the administration of those remedies which relax the surface of the body, and promote the cuticular discharge, are particularly indicated; these kept up for some time are found to be the most efficient means in the cure of yellow fever."

But Dr. F. does not consider the *abuse* of mercury as confined to the treatment of the yellow fever; but remarks that recourse is had to the same remedy frequently on improper principles for the cure of all the various forms of *intermittent* and *remittent fever* which prevail in different parts of our country, and for every variety or type of fever, from the *purest inflammatory* to the *lowest grade of typhus*. The doctor places in a striking point of view the very contradictory indications that are said to be answered by the same remedy in this plan of practice.

He concludes his Essay by treating of the exhibition of mercury in the treatment of lues venerea, and remarks, that it may well be questioned, whether the maladministration of the remedy has not produced as destructive consequences as the disease itself; quoting John Hunter's observation, "that it is nearly as dangerous in many constitutions to give mercury, where the disease is not venereal, as to omit it in those which are."

On this subject our author displays practical knowledge and extensive reading, and sets himself decidedly in opposition to

what he calls the common and most destructive error of inducing profuse salivation by throwing into the system large quantities of mercury in the cure of syphilis. When the discharge is thus excited, says Dr. F., it often continues until a total exhaustion of the strength of the patient ensues, whilst in many cases where it has been thought to have removed the disease, it has been found to produce only a temporary cessation; and in other instances it has converted a comparatively mild disorder into one infinitely more dangerous.

As he supposes that the venereal virus contaminates the system by an *assimilating process*, so the most certain and effective practice in its expulsion or cure, depends upon producing an increased discharge from *the excretory vessels of the whole system*.

To obtain this end then, mercury, as is universally acknowledged, possesses superior claims; and those preparations of it, as Dr. F. observes, which more directly act upon the secretory vessels of the surface, for the reasons before mentioned, are to be preferred. And hence the *murias hydrargyri*, or the corrosive sublimate, is recommended for these several purposes. After mentioning the distinguished names of many of its advocates, he observes, that among the principal advantages it possesses over every other preparation of the same remedy are, that, judiciously administered, it is particularly mild and safe in its operation, will admit of more extensive use in all the various forms of lues venerea, and subject the patient to fewer inconveniences; that it readily enters into the general circulation, becomes miscible with the several fluids of the body, the soonest arrests the progress of the complaint, and eliminates the morbid matter through those emunctories best calculated for that purpose; that it supersedes the necessity of salivation, by its action on all the secretions and by promoting especially the cuticular discharges, and the evacuations from the kidneys: that it is the only preparation to be depended on in those peculiar habits of body so susceptible to become salivated by every other form of mercury now in use; that in its ultimate effects upon the constitution, it is attended with comparatively no injury.

In confirmation of the valuable properties of this preparation

of mercury in the cure of syphilis, the testimonies of Dr. Locher of the Vienna hospital, of Van Swieten, and of Sir J. Pringle are quoted.

As cooperative with the murias hydrargyri, our author says that the lignum guaiaci and the radix sarsaparilla seem to claim the first notice.

This mode of treating the disease, we are informed, has been attended with the greatest success in the New York state prison, hospital, and almshouse, under the superintendence of Dr. Hossack; and in those cases in the New York hospital the external use of every preparation of mercury was omitted; instead of them the lunar caustic was occasionally used.

In the above analysis our object has been to give a plain and concise summary of a thesis which we think deserving the attention of practitioners. Notwithstanding we do not mean to be considered as embracing all the doctrines it contains, some of which, although supported with considerable ability, are in opposition to the greatest medical authorities.

MEDICAL INTELLIGENCE.

Vaccine Society.

The Society continues its attention to the vaccination of the poor in the city and districts. Twelve hundred and seventy-seven persons have passed through the process successfully since the last notice in the Eclectic Repertory. The mode of finding out proper objects by the collectors, who call at their houses, and report them to the physicians, has been found to be at once efficient and satisfactory.

Humane Society.

It may be useful at this period to remind our readers that the society's premiums " for the best dissertation on the means of restoring to life persons apparently dead by drowning, and more effectual than any yet in use," will be adjudged after the first day of January, 1813; until which time dissertations will be received by the secretary, Isaac Snowden, of Philadelphia.

Philadelphia Dispensary.

From December 1, 1810, to December 1, 1811, there have been three thousand, three hundred and fifteen patients under the care of the institution.

Remaining under care from last year, - -		98
Admitted since that time, - - -		3217
		3315
Of these the number cured is, - -	3013	
relieved, - - -	92	
dead, - - -	101	
removed, - - -	14	
irregular, - -	25	
remaining under care, -	70	
		3315

The receipts of moneys of the institution during the year, including last year's balance in the treasurer's hands, have been, - - -		Dolls. 2947 56
The expenditures have been,	$ 2947 47	
Balance in treasurer's hands, Dec. 11, 1811,	0 09	
		2947 56

THERMOMETRICAL OBSERVATIONS.

State of the weather at Philadelphia for the last six months of 1811—the degrees of heat by the thermometer being taken at three o'clock, P. M.

JULY.

Thermometer—	Lowest	72
	Highest	93
	Mean	80

Winds—Northerly during the greatest heat, with clear weather from the third to the ninth of the month, both included. The remainder of the month, southerly winds.

July was hot and dry in the beginning of the month—very favourable for harvest, which was abundant and well got in: plentiful and frequent showers afterwards. Much damage to the northern and southern parts of our country by rains in this month. From the third to the ninth, many deaths in New-York and Philadelphia, from heat and from drinking cold liquors, when the body was warm.

AUGUST.

Thermometer—	Lowest	66
	Highest	86
	Mean	73

Winds—Northwest and southwest, the greater part of the month; the latter part, easterly.

The month was pleasant—on the whole rather cool—very advantageous for the fruits of the earth, by occasional rains—not so much thunder and lightning this summer as usual.

SEPTEMBER.

Thermometer—	Lowest	59
	Highest	83
	Mean	70

Winds—Chiefly westerly.

A moderate month on land, but severe weather on the coast. A comet was seen about the beginning of the month in the

N. N. W. It then appeared to set about a quarter past eight o'clock P. M., and at the end of the month at past nine o'clock.

October.

Thermometer—Lowest	. . . . .	43
Highest	. . . . .	79
Mean	. . . . .	60

Winds—Variable; more easterly and westerly than from any other point. The month was moderate. Not much rain: scarcely any frost. The comet has progressed considerably; apparently from N. W. to S. E. and by the end of the month appeared to set at ten o'clock.

November.

Thermometer—Lowest	. . . . .	34
Highest	. . . . .	56
Mean	. . . . .	45

Winds—Mostly westerly.

A very moderate month. Little frost or ice. No snow. The comet appears to continue its course southeasterly.

December.

Thermometer—Lowest	. . . . .	18
Highest	. . . . .	52
Mean	. . . . .	36

Winds—Westerly winds prevalent.

On the 14th, snow two or three inches deep. On the 22d and 24th, snow from one to three inches deep. The navigation of the Delaware obstructed by the ice on the 21st, and continued so occasionally to the end of the month. The comet seen for the last time on the 24th, apparently taking a southwest course.

On the 16th and 17th an earthquake was felt from Georgia to Lake Erie.

Dr. Portal of Paris, has proved by numerous observations, that the signs usually admitted as distinguishing the sanguineous from the serous apoplexy, are illusory. He distinguishes apoplexies according to the causes, dependent either on the

disposition of the body, or on external circumstances; and demonstrates from his own experience, and from that of the most eminent practitioners in all ages, that bleeding holds the first rank among the remedies opposed to this formidable disease. The public may expect the appearance of this work before long.

Dr. Dumas, Dean of the Faculty of Medicine at Montpelier, having observed that a person subject to epilepsy had the paroxysms accelerated by the use of intoxicating liquors, directed them with the view of giving a periodical character to the disease, and then gave the cinchona with the happiest effect.

The French Chemists have been actively engaged in the analysis of animal and vegetable matter: at the head of the list we discover the names of Berthollet, Gay-Lussac, Thenard, and Vauquelin. Vauquelin observes that the difference between common sugar, the sugar of milk, and gum, does not merely consist in the presence or absence of azote, but in the various proportions of the other elements; to ascertain which he has commenced a series of experiments.

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SELECTED PAPERS.

Observations and Experiments on Pus.

By GEORGE PEARSON, M. D. F. R. S.*

[From Nicholson's Journal, for 1811.]

CHEMICAL writers vary in their statements of the properties of pus; and they consider, that a farther investigation is requisite for the purposes of science. Physicians confess, that, in numerous cases, they cannot form a satisfactory judgment of the nature of diseases, on account of not being able to determine what is, and what is not purulent matter; likewise probably, on account of the existence of different kinds, or varieties, at least, of this substance, afforded by different disorders.

I beg leave, therefore, to submit to this learned society, my own observations, experiments and reasoning on this animal matter.

SECTION I. *Simple, and obvious Properties.*

The different kinds of fluid, commonly considered to be pus, may be distinguished by the following titles:

I. The creamlike and equally consistent.

II. The curdy and unequal in consistence.

III. The serous and thin kind.

IV. The thick, viscid or slimy.

* Philosophical Transactions, for 1810, p. 294.

I. A pint of the first sort was taken out of the pericardium, after a fatal inflammation of the heart, in St. George's hospital, and obligingly sent to me by my colleague, Dr. E. N. Bancroft.

The *colour* was yellowish—the *smell* was fleshy when warmed—it was smooth and unctuous to the touch.

2. The specific gravity of two different portions was as 1630 and 1633, that of distilled water being 1580; each substance being of the same temperature. Serum, of the blood of different patients, was found at the same time to be 1626, 1627, and 1630. Accordingly, the distilled water being 1000, the pus is 1031, and 1033; and the serum is 1029, and 1031.

3. After twelve hours repose, about two ounces by measure of a limpid fluid having appeared on the top, it was decanted from off the opake purulent fluid; which was become thinner in the upper part of the vessel containing it, and thicker in the lower than before.

4. On farther repose, it did not become offensive so soon as a portion of the same pus mixed with a little blood, or as serum alone.

5. This pus neither indicated acidity nor alkalescency to the usual tests; viz. turnsole paper, tincture of red cabbage, Brazil-wood paper, and turmeric paper. I have, in other instances, sometimes observed acidity to be indicated by turnsole paper; but in none alkalescency, so long as the matter remained without fetor.

6. Being examined under the microscope, when duly diluted with distilled water, innumerable sphericle particles were seen, which did not appear altered in figure, or diminished in number, by extreme dilution; that is, they did not appear to have been dissolved.

II. A pint of pus of the second kind, viz. *curdy*, was afforded by a psoas abscess.

The colour was brown. It felt knotty. On pouring from one vessel to another, the curdy masses were manifest, and of various sizes, from that of a pin's head to a hazel nut. It was more viscid than the former, and of a little greater specific gravity. On standing, a limpid fluid appeared upon the top, as in the first kind, but in smaller quantity. Globules were seen with the microscope, but also a number of irregularly

figured larger masses. Putrefaction took place sooner than in the former kind. In other properties, this pus was similar to the first kind.

III. *Serous thin pus.* It was produced by a fatal inflammation of the peritoneal coat, without ulcer, and taken out of the cavity of the abdomen. A good deal of serum was also effused, of which the pus was a deposit. It was not much thicker than milk. To the feeling it was not at all unctuous. The smell was slightly offensive. On standing twenty-four hours, a sediment appeared, occupying only one half the full vessel, under a wheylike liquid. Putrefaction took place sooner than in either of the two former kinds. The specific gravity was the same as that of the first sort. In other properties it was similar to the creamlike pus above distinguished.

IV. A pint of the *viscid pus* was obtained from an abscess among the muscles of the thigh. If I had not had entire confidence in Mr. Brodie's accuracy, who was so obliging as to attend to my request, on this and many other like occasions, I should have supposed, that this was expectorated matter, it so exactly resembled in its simple properties the *ropy kind*, described in a paper on expectorated matter. Phil. Trans. 1809, P. ii. p. 317.

The appearance was not quite uniform, there being semitransparent masses in small proportion, mixed with the perfectly opake white matter. It was almost inodorous. To the touch it was quite smooth. The specific gravity was nearly that of the second kind of pus.

On standing twenty-four hours, about one ounce measure of limpid fluid rose to the top of the whole mass. Putrefaction did not take place so soon as in expectorated matter of the same consistence.

The examination by the microscope manifested innumerable spherical particles among leafy masses, and numerous particles of irregular forms.

The simple properties were otherwise similar to those of the other sorts of pus, above distinguished.

Many other differences of purulent matter are universally recognised; but they are either varieties of the four kinds already named, or the differences depend upon the obvious mix-

ture with adventitious substances; such as the red part of the blood, coagulated lymph, serum, putrefied matter, fibrous and membranous masses, calculi, &c.: therefore, I deem it useless to describe them.

SECT. II. *Agency of Caloric.*

1. The above kinds of pus coagulated like serum of blood into a firm, uniform, soft solid, at the temperature of 165° completely; but partially at 160° of Fahrenheit's thermometer.

2. The decanted limpid fluid from pus, sect. I. II. III. IV. coagulated completely into a firm uniform mass, like serum of blood, at 165°, but it became opake and thickened at 160°. By pressure of the firm curd thus produced, a watery liquid was separated, which on due evaporation did not give a jelly, but was coagulable like the decanted liquid just mentioned.

The thick opake matter, after decanting the limpid fluid, coagulated as before said, into a firm mass at 165°.

3. Each of the above four kinds of pus, being evaporated to dryness, left in no case less than one tenth of its original weight, or more than one sixth; but most frequently one seventh or one eighth of brittle matter. The smallest proportion of residue was left by the 3d, or serous kind; the largest, by the 2d or curdy. These residues generally became rather soft, especially those of the 3d, or the serous kind, after exposure to the air.

4. The opake part of pus, after separating the limpid fluid, afforded on evaporation from $\frac{1}{18}$ to $\frac{1}{30}$ more of brittle residue, than an equal weight of the pus itself; and it remained hard on exposure to the air. The limpid fluid, evaporated to dryness, yielded about one tenth of brittle residue, which grew moist, and sometimes deliquesced, on exposure to the air.

5. The brittle residues above mentioned (3), being exposed to fire in platina crucibles, flamed for some time, emitting a very offensive, pungent, empyreumatic smell; the uninflammable residue being kept in a state of ignition for a longer period, what remained at length was fused readily from the *serous*, viz. the third kind of pus; but in the cases of the other exsiccated residues of the 1st, 2d, and 4th kinds of pus, they barely were melted, or only became soft and claggy. The

fused residues from the *serous pus* amounted to $\frac{1}{30}$ or $\frac{1}{35}$ of the exsiccated pus; and to $\frac{1}{250}$ or $\frac{1}{300}$ of the original purulent matter. Those from the second kind, the *curdy* amounted to $\frac{1}{50}$ or $\frac{1}{60}$ of the dried matter, and to $\frac{1}{350}$ or $\frac{1}{00}$ of the pus itself. The fused masses from the 1st and 4th kinds of purulent matter afforded intermediate quantities of melted matter between those just mentioned.

6. The fused residues (5), being treated in the manner described in a former paper, Phil. Trans. 1809, P. ii. p. 326. 329, I found they consisted chiefly of muriate of soda, phosphate of lime and potash; with strong indications of carbonate of lime, and a sulphate; beside traces of phosphate of magnesia, oxide of iron, and vitrifiable matter, probably silica. On a reasonable calculation, it appeared, that in the *serous kind* of pus, the muriate of soda amounts to from one and a half, to two per 1000; the phosphate of lime from one, to one and a half per 1000; the potash from one half, to three fourths of a part in this quantity; and the other matters together, to half a part in 1000. In the *curdy matter*, the second kind, the muriate of soda amounts to from three fourths of a part, to one in 1000; the phosphate of lime to one; the potash to less than one half; and the other matters united, to a half part in 1000. The first kind of pus, the *creamlike*, and the fourth the *viscid*, afforded from the melted residue the same substances as the *serous* kind, excepting a somewhat smaller proportion of muriate of soda, and potash.

7. The brittle residues of evaporated pus, after decanting the limpid fluid (4), being treated with fire as above related, the remaining matters were melted with more difficulty, and less completely, and contained a smaller proportion of muriate of soda and potash than the original pus.

8. The decanted limpid fluids (4), being evaporated to dryness, these residues were exposed to fire. They were melted, and then afforded a larger proportion of muriate of soda and of potash, than the pus itself; but with the same proportion of the other saline and earthy substances.

SECT. III. *Agency of Water.*

1. After decanting the limpid fluid from off half a pint of the four kinds of pus as above related, (sect. I.) three ounces by measure of distilled water were mixed with each of them. After forty-eight hours' repose, a limpid fluid of nearly the quantity of two ounces by measure was seen forming an upper stratum to the pus. It was decanted for examination.

(*a*) On exposure to fire it became turbid like milk, as soon as the temperature was elevated to 165°, but did not become thicker at a greater elevation.

(*b*) On evaporation to dryness, the residue amounted to about one fifteenth of the weight of the liquid from the serous pus, and to one twentieth from the three other kinds; in place of about one tenth, as from the first decanted liquid, (Sect. I, 4); and as from serum of blood. The residuary matters were of the same kind as those above described, Sect. II, 2—6.

(*c*) Three ounces by measure of distilled water having been again mixed with each of the four kinds of pus, and, in forty-eight hours, two ounces measure of decanted limpid fluid from each having been evaporated to dryness, residues of the same kind, in the same proportions, and in nearly the same quantities as before, were obtained (*b*). These decanted fluids became nearly as turbid as the former, on raising their temperature to 165°.

(*d*) Distilled water was added a third time, in the quantity of eight ounces by measure, to each of the four parcels of pus under examination; and, after forty-eight hours' repose, six ounces of limpid fluid were poured off from each of them. At the temperature of 165°, the decanted fluids became turbid, that of the serous pus more so than the others. On evaporation to dryness, a much smaller quantity of residue was obtained than before, viz. one sixtieth from the serous pus, and one seventieth from the others; and it consisted of the same kind of substances as above described; but the muriate of soda and potash were in smaller proportions than before.

(*e*) A fourth time distilled water, in the quantity of a pint, was mixed with the present four parcels of pus; and after standing forty-eight hours, three fourths of a pint of clear

colourless liquid was poured off from each of them. It became slightly turbid and whitish on boiling. On evaporation, each parcel afforded about $\frac{1}{90}$ of the fluid employed. The residues now consisted of animal matter, with a much smaller proportion than before of muriate of soda, phosphate of lime, and potash—nothing else could now be traced.

(*f*) Distilled water, in the quantity of a pint, was once more mixed with the four sorts of purulent matter undergoing inquiry. After forty-eight hours, a pint of liquid was decanted from off each of them; but being slightly turbid, they were left to stand twenty-four hours. By this time a sediment was deposited from each of the liquors; but being still, though very slightly, turbid, they were filtrated through suitable paper. They were then transparent. The transparent filtrated liquors had their transparency disturbed by a boiling temperature. They became also slightly milky with a nitrate of silver, but scarcely so with infusion of gall nut. On evaporation to the quantity of an ounce from each pint, the residuary liquids appeared slightly globular. These, on evaporation to dryness, yielded not more than one part of animal matter, from each 500 of the transparent filtrated liquids.

(*g*) On standing three or four days in a cold room, the parcels of pus, after the ablutions just related (*a*—*f*), exhibited a whey coloured liquor at the top, of which about $\frac{1}{4}$ of a pint was poured off from them. More turbid liquor was also separated from the washed pus, by pouring it upon a porous cotton cloth strainer, which left purulent matter of the consistence of starch mucilage, amounting to about one half the original weight.

(*h*) The pus freed from coagulable limpid liquid by repeated ablutions (*a*—*h*) was white as snow—equal in consistence—perfectly smooth—the fourth kind was less viscid than before, but the others were more so—no smell—not at all disposed to putrefy—on elevating its temperature to 165° and higher, it did not coagulate into one mass, nor into clots, or large masses of curd, but a watery fluid separated from a fine soft somewhat curdlike opake fluid; which did not become more curdy, even on boiling—it did not appear that above a grain of this part, or state of pus, dissolved in 1000 waters—was highly globular

under the microscope, and remained so, although coagulated by nitrate of silver; by infusion of gall nut; by alcohol; and supersulphate of alumina—with muriate of ammonia, nitrate of potash, and other neutral salts, and with carbonate of potash, it produced a viscid semitransparent mass like expectorated half transparent matter—exposed to fire in a platina crucible, it was inflamed, but did not omit an offensive smell, and after continuing the ignition, the residue was a particle of half fused matter, not amounting to $\frac{1}{1000}$ of the pus after ablution, nor above $\frac{1}{300}$ of the same matter exsiccated; it consisted of phosphate of lime and vitrified matter—no ammonia was perceivable, on mixing lime with this washed pus; nor muriatic acid on adding sulphuric acid.'

2. (*a*) A teaspoonful of the *creamlike pus*, being agitated in half a pint of distilled water, produced a milky fluid, with a number of small curdy particles suspended, but very few leafy or fibrous pieces or clots.

(*b*) The *serous pus* being treated as just mentioned (*a*), the same appearances ensued.

(*c*) The *curdy pus* being agitated in the same manner in water, a number of clots, leafy, and fibrous masses, were seen suspended among fine small curdy particles in a pearly liquid.

(*d*) The *viscid pus* being treated as just said, it required long continued and violent agitation, to diffuse it through the water, and then the appearances were as last described.

3. Pus of any kind, after boiling in twenty times its quantity of water, was quite as globular under the microscope as previously. With a smaller proportion of water, the mixture became very turbid, sometimes clots were formed in a pearl liquid, in which a fine sediment took place, which appeared much more globular than the clots or curdy masses.

4. In general, water in which pus has been agitated remains somewhat milky, with an abundant close white sediment; but after two, or three, or more ablutions, the water becomes clear on standing, and the sediment more curdy.

SECT. IV. *Agency of Alcohol of Wine.*

The different kinds of exsiccated pus exposed to the agency of this menstruum, and treated as described in a former paper,

Phil. Trans. 1809, P. II. p. 329, the results were similar, except in the proportion of products.

1. These exsiccated substances afforded to this menstruum a smaller proportion of potash, but as much animal oxide and muriate of soda, as mucous sputum.

2. The undissolved matter left after repeated digestions in this menstruum afforded the same substances, but in smaller proportions, as mucous sputum.

3. Equal bulks of fresh pus, and rectified spirit of wine, afford a much thicker and more milky liquor, with a closer sediment, than expectorated mucous matter.

SECT. V. *Agency of acetous Acid.*

The purulent matters mixed with this acid became curdy, and rendered it milky; but on standing, a close white sediment appeared, the liquid above being clear, except in the case of the viscid pus, which exhibited leafy and fibrous masses, as hath been described with mucous sputum.

By repeated digestion of the different kinds of pus in this menstruum, I obtained the same results, except the proportions of acetite of potash, and muriate of soda being smaller, as related in a former paper on mucous expectorated matter. Phil. Trans. 1809, P. ii. p. 336.

SECT. VI. *Some Experiments with different Objects, especially to distinguish Pus and Mucus.*

1. In the agency of sulphuric, nitric, and muriatic acids, in sufficient quantity to dissolve and decompound the substances under inquiry, I could perceive no important difference between them. The purulent matters indeed required a much greater proportion completely to dissolve them, than the transparent sputum. Also, the more opake and dense the sputum, the greater the resistance to dissolution. Sulphuric acid produced black liquids like those containing charcoal, smelling strongly of muriatic acid; but on dilution with water, they became clear. No precipitation occurred on dilution with water, and on saturation with the fixed alkalis, but a trifling sediment appeared, which redissolved on the addition of the above acids.

2. The mineral acids diluted, or added in small proportion, and the vegetable acids, coagulate variously pus and mucous fluids. Some become merely milky fluids, others curdy fluids, others afford fibrous and leafy masses in a transparent liquor, and others give a uniform thick mass of curd. On standing, the deposits are accordingly of various forms, and the liquors above of various appearances; but I could discover no constant characteristic property of the substances by these experiments, as some writers have asserted.

3. The solid fixed alkalis, or lime, mixed with expectorated mucus, occasion a stronger smell of ammonia than with pus; or than with muco-purulent sputum. Some use may be perhaps made of this easy experiment to judge of the nature of varieties of the fluids in question, particularly as far as depends on the proportion of ammonia; for sometimes it cannot be perceived by the smell on mixing alkalis, but can by muriatic acid giving white vapours. Concentrated liquid alkalis, added to both pus and mucus, dissolve them to produce clear liquids, except small curdy parts and motes. These curdy parts and motes resist dissolution also for some time even in nitric acid, and seem to be self-coagulated lymph. They are in much greater proportion in pus than mucus. The addition of acids to these alkaline dissolutions occasions precipitations: but no differences, or not with sufficient uniformity to afford criteria, were observed according to the observations of other experimenters.

4. Concentrated aqueous solutions of various neutral salts, viz. muriate of ammonia; nitrate of potash; muriate of soda; sulphate of soda, &c.; being mixed in due quantity with pus of the kinds under examination, produce viscidity, like ropy expectorated matter, thickening like jelly, and less opacity. These changes have, in the case of muriate of ammonia, been called coagulation by Mr. Hunter; but by agitation in cold water the matters are diffused, and on standing, the pus is precipitated in its original state. I call these effects of the neutral salts inspissation, seemingly occasioned by their attracting water from the pus; for no such change is produced if either the purulent matter, or solution of salts, be diluted; nor is it produced if the pus be previously coagulated by caloric: also

the inspissated pus is coagulable by caloric as usual. No such inspissation is produced by these salts in mucous sputum, or in muco-purulent sputum, so that undoubtedly it is a criterion as discovered by Mr. Hunter in the case of muriate of ammonia, and with other neutral salts, as now manifested.

5. I endeavoured to find some easy tests for distinguishing pus from mucus; but I did not succeed with the tanning principle; gallic acid; supersulphate of alumina; nitrate of silver, and other metallic salts; and as already said, various acids. They all produced precipitation of these animal matters, but not with observable characteristic differences.

6. To observe the state in which the matter of pus is secreted, I procured the assistance of Mr. Maynard, the present house-surgeon of St. George's hospital, and Mr. George Ewbank, who had been on many occasions essentially serviceable in my inquiries. Square pieces of goldbeater's skin were applied to various sore legs after carefully removing the matter already secreted. In five or ten minutes the square pieces being removed, they were found wet with a limpid fluid. In this state they were inspected by the microscope, by which numerous globules were seen. In ten minutes farther the liquid was no longer limpid but opake, like pus, in which the usual sphericle particles were seen with the microscope as just mentioned.

Supposing objections might be offered on account of the alteration of texture of the skin employed, square pieces of glass were also applied. The results were the same in both trials. The two gentlemen above named, as well as Dr. Richard Harrison, and other pupils, who happened to be present, all concurred in the observation, that the limpid matter became opake, and that while limpid it was, like pus, full of spherical particles.

The statement of the properties of pus in the foregoing inquiry I hope will be found to be true; and I submit to the judgment of others whether or no the following inferences are legitimately established.

1. That this fluid essentially consists of three distinct substances, viz. 1. An animal oxide, which, among other properties, is distinguished by its being white, opake, smooth, of the

form of fine curdy particles in water; not dissoluble in less than 1000 cold waters; not coagulable into one mass like serum of blood by caloric, alcohol, &c.; only rendered more curdy by water from 160° to 170°; but readily diffusible.—2. A limpid fluid resembling serum of blood in its impregnations, and in its coagulability by caloric, alcohol, &c.; in which the opake oxide is diffusible but not dissoluble, and which is specifically lighter than that oxide.—3. Innumerable spherical particles visible only by the microscope in this opake oxide, and in small number in the limpid fluid; not coagulable by any temperature to which hitherto exposed, and not destructible by many things which combine or destroy the opake oxide; and these globules are specifically heavier than water.*

2. That the *visible* curdy masses, as well as the fibrous or leafy parts, almost always contained in smaller or larger quantities in pus, may be considered as self-coagulated lymph, which in its fluid state is secreted without having the state of aggregation produced in it like that of the *essential* opake oxide of pus.—Sect. VII, 1.

3. That the reddish, the blackish, and the dark brown colour of pus depends upon the red part of the blood effused or secreted from the same vessels, or from contiguous ones which secrete pus.

4. That on some occasions the clotty and irregularly figured masses found in the pus may depend upon disorganization or breach of the contiguous solid parts.

5. That whenever pus is fœtid to the smell, a portion of it is in the state of putrefactive fermentation, which may be removed by ablutions with water.

6. That there are certain adventitious matters liable to be contained in pus not hitherto rendered palpable to the senses, but known by their effects in exciting contagious diseases; such as small-pox, syphilis, &c. These matters are produced by a specific action in the secretory organs of pus, by such matters

* My obligingly attentive pupils, Mr. Burton, and Mr. Stansfeld, house-surgeons of the Lock hospital, collected for me a sufficient quantity of gonorrheal matter to determine, that it consisted of the three ingredients here stated.

themselves either contained in the circulating blood, or on the secreting surface.

7. That the *essential* substances of which pus consists, as well as some of the adventitious ones (Sect. VII, 1, 2, 3, 6.) are separated from the blood by a peculiar organization belonging, or attached to the bloodvessels: which organs of separation or secretion are not only excited to the action which produces pus in diseased states, but they are evidently influenced by the states of other distant organs of the animal economy; hence many varieties in the properties of the purulent matter.

8. That the varieties of purulent matter relate to differences of *quantity*—the proportion of the essential substances (1)—and the adventitious parts (2, 3, 4, 5, 6,). The *cream-like* pus consisting of almost purely the opake oxide and limpid liquid (I, 1, 2,). The *curdy* containing a large proportion of coagulated lymph, or broken down solids. The *serous* abounding in limpid fluid. The *viscid* depending upon the coagulation, and perhaps, inspissation, by union of neutral salts with the opake oxide.

9. That as the essential parts are secreted in a limpid state, but presently become opake, owing to a large proportion spontaneously coagulating, and thus becoming the opake oxide, mixed with the serous liquid, and innumerable spherical particles (Sect. VII, I, 1, 2, 3,) it seems reasonable to infer, that these matters are the self-coagulated lymph of the blood and serum, separated by the secretory organs; which act of secretion determines the subsequent state of aggregation of pus, and the globules are at the same time formed analogously to their formation by other secretory organs. How far they are those of the blood altered by secretion may be determined hereafter. It is a collateral proof of this inference, that very thick pus affords from one sixth to one seventh of exsiccated brittle residue, which, as I have found, is nearly the same proportion afforded on the exsiccation of the buffy coat of inflamed blood; while very thin pus affords on exsiccation from one eighth to one eleventh of brittle residue, which is the proportion to be expected from a mixture of serum of blood and self-coagulated lymph, as I have ascertained.

10. That the constant impregnating saline and earthy ingredients of pus are dissolved in the serous fluid; and are all seperable along with the serum, by ablutions with water, from the opake oxide (1), except a portion of the phosphate of lime. These impregnations are the same as those of serum of blood, and of expectorated mucous matter, viz. muriate of soda; potash neutralized by animal matter or a destructible acid; phosphate of lime; ammonia neutralized probably by phosphoric acid; with a sulphate, and traces of some other matters mentioned in my former paper. The proportion of these impregnating substances is as the proportion of limpid or serous coagulable fluid, and of course inversely as the proportion of the opake oxide of pus; but it varies in different cases in given proportions of this oxide, and the limpid fluid. In general, if not always, a given quantity of pus contains a smaller proportion of saline matters than an equal given quantity of expectorated mucous matter, but a given quantity of the limpid coagulated fluid contains a greater proportion of saline matters than an equal given quantity of serum of blood. Hence the thicker the pus the less irritation to the sore which secretes it, and commonly the less the inflammatory or other action of the secreting surface. In different cases, however, the proportion of impregnating saline substances to one another is liable to vary, especially that of phosphate of lime; hence, though rarely, calculi occur of this substance in the cavity of the abscess.* Hence too the exsiccated pus is liable to become soft and moist, from the proportion of neutralized potash being greater than usual; and even deliquescence sometimes occurs of the exsiccated limpid fluid.

12. That the same organs, according to their different states, secrete from the blood merely water impregnated with the saline substances of the serum of blood; also this fluid con-

* On examining the lungs of a patient who died of pulmonary consumption, concretions were found in a large vomica from the size of mustard seed to a pepper corn, which Dr. E. N. Bancroft reserved for my inquiry. I found they consisted chiefly of phosphate of lime, with an unusually small proportion of animal matter. In another patient of Dr. Nevison, matter was coughed up, consisting chiefly of phosphate of lime and animal matter, nearly one of the former to three of the latter.

taining various proportions of coagulable matter like that of serum of blood; and serous fluid with self-coagulable lymph, which affords curdy masses: likewise this serous fluid, together with this matter which coagulates of itself after secretion, highly impregnated with invisibly small particles, in such a state of aggregation, as to constitute the thick opake fluid called pus—which states of the secretory organs are generally attended with inflammatory action, but frequently also without any symptoms of such action.

13. That beside the consistence of pus depending upon the proportion of serous limpid liquid, and opake matter, it also probably depends upon the mode and state of coagulation of the matter which affords this opake part; analogously to the different states of consistence of the coagulated blood itself, according to the different conditions of the animal economy.

According to the above inferences, I trust, a distinct and definite notion of the substance to be considered as pus is exhibited; and I do not comment on the different results of experiment and conclusions of other writers, because future observers only can determine the truth. What is and what is not pus will now readily be ascertained by a few easy experiments; by the obvious properties; and by the consideration of the source of the matter in question: provided, however, that it be unmixed with certain other matters, by which disguise is produced. As already observed it is in pulmonic diseases that the ambiguity occurs; and physicians lay very considerable stress upon the nature of expectorated matter in their practice and reasoning; I shall therefore endeavour to elucidate the subject by remarks on the puriform matter expectorated in different cases.

1. An abscess occasioned by acute inflammation not only of a pleurisy, and peripneumony, but of other diseases which have not the symptoms of any one which has received a designation. Here there ought to be no doubt; for the matter which is coughed up suddenly and abundantly on the bursting of the abscess is evidently pus with little mucus. Such matter consists of the essential ingredients of pus, (Sect. VII, 1,) with generally the adventitious substances, (Sect. VII, 2, 3, 4,)

viz. coagulated lymph, membranous or fibrous parts, and a small proportion of the red part of blood.

2. Purulent expectoration from the rupture of abscesses, or vomicæ of suppurated tubercles. In such cases there has been a chronical cough with viscid sputum, commonly in persons of an advanced age. After this long continued disease, an abundant expectoration of quite a different kind from the former suddenly comes on; by which the patient often dies very speedily; sometimes immediately, being seemingly choked. This kind of matter evidently consists chiefly of the essential ingredients of pus, (Sect. VII, 1.) with not only the adventitious substances, viz. clots of self-coagulated lymph, and sometimes the red part of blood, but also masses, which are apparently the broken down solid parts, the cellular membrane, the vessels and substance of the tubercles, in a disorganized state. The sufferer often says, such matter tastes sweet. The mucus is here in too small a proportion, and not intimately mixed, to occasion disguise.

3. In the bronchitis, or inflammatory affection of the air tubes, the membrane remaining entire, attending various diseases, e. g. the measles, a fever with a cold, various continued fevers, an expectoration of thin creamlike matter occurs, at first gradually, but at last in great quantities, continuing for a week or more. Although mucus is usually coughed up with this puriform substance, the two things generally remain in distinctly large masses. With little skill the opake or puriform fluid may be collected separately from the mucous matter. It will be found to consist almost purely of the three essential constituents of pus (Sect. VII, 1.) there being seldom any adventitious substances.

4. Muco-purulent, or commixed expectorated matter. This kind is perhaps of the most frequent occurrence. It is that which many physicians know not how to designate; some consider it to be pus, and others to be mucous matter. This contrariety of opinion arises from the want of definite notions of pus and mucus. Hence the parties are not able to perceive, that in this kind of sputum exist many of the properties of pus, and also of mucus. I have described it in my former

paper on expectorated matter, Phil. Trans. 1809, P. ii. p. 317, under the denomination of *opake ropy matter*, the third kind. I feel no degradation in finding it necessary to confess, that a better acquaintance with the properties of pus has taught me, that I was in an error, in considering this kind of expectorated matter to differ from other sorts merely in the proportion, and not in the kinds, of constituent parts. It now appears that the sputum in question possesses such properties as might be predicted to exist, from the known properties of pus and mucus separately, in case these two substances should be intimately commixed. Accordingly, the opacity; the straw colour; the greater density than mucus; the great globularity under the microscope; the greater proportion of residue on evaporation to dryness, than from mucus; the milky liquid on heating this matter; the milkiness on agitation in cold water; are properties of pus. But the great viscidity, yet not increased by neutral salts; the less opacity than pus; the less globularity than pus; the smaller proportion of exsiccated residue than from pus; the moisture, or greater moisture on the exposure of the brittle residue to air, than from that of pus; the more difficult diffusibility through cold water, and less degreė of milkiness than from pus: the great proportion of leafy or fibrous masses on agitation in a very large quantity of cold water; the speedy putrescency; are properties of mucus. The mode of coagulation by caloric at 160° and upwards is such as might be expected from the commixture, viz. in large masses of curd in a milky liquid, instead of into one uniform mass like pus, or into small curdy masses in a very large proportion of a whey coloured liquid, like mucous sputum. Thick pus affords on evaporation to brittleness, $\frac{1}{7}$ or $\frac{1}{8}$ residue; and transparent sputum of the consistence of jelly, gives about $\frac{1}{18}$ or $\frac{1}{20}$ of such residue: but this opake matter under inquiry, affords $\frac{1}{16}$ or $\frac{1}{15}$ of brittle residue, according to the proportion of the two substances. I could not separate the supposed pus and mucus from one another, to exhibit them distinctly by water, or by any other means, on account, as I conceive, of the intimate diffusion through one another, and their mutual cohesion. But on evaporating the milky water, produced by agitating this sputum in it, or by letting it stand to collect the sediment, little else

besides a mere congeries of globules seen under the microscope was thus obtained. For the same reason, on standing, a serous liquid like that of pus (Sect. VII, 1.) does not separate, or only partially, from the opake part, so as to render it possible by ablution, to collect this coagulable liquid like that of pus: and the greater proportion of water, belonging to the mucus, occasions the coagulation by caloric, to afford only a milky liquid, instead of a uniform mass of curd.

This kind of sputum, consistently with the phenomena, must be produced by secretion from the bronchial membrane in its entire state, and not by ulceration or abscess. For it is secreted in many cases, at the rate of a pint or more in each twenty-four hours, for weeks and months successively, and for twenty or more successive winters. Also many persons recover their good health after this secretion, and it is the usual termination favourably of pneumonia, bronchitis, &c. It is produced by any disease of great irritation of the lungs; as I have found from ossification of the bronchial or pulmonary arteries: from calculi: from broken wind, or rupture of air cells, &c.*

It is secreted also in consequence of irritation of the bronchial membrane by tubercles, vomicæ, water in the cavities of the chest, &c. The same kind of matter is secreted from the nose on the decline of a common severe coryza in many cases. It appears then, that this kind of matter is a symptom of the most fatal, as well as harmless diseases—it is a symptom in one case of the progress of disease to death, and in another of the termination in health, by being seemingly a critical discharge. Perhaps, if these facts had been observed and considered, numerous mistakes in prognostics would have been avoided, and better practice have been employed; because the nature of diseases would have been rightly understood. From this representation it is plain, that a just opinion cannot be given merely from the examination of the sputum, without considering the disease by which it is produced, or of which it is a symptom.

* I believe this state of the lungs to have been first ascertained in broken winded horses, by Mr. Colman.

The proportion must also be considered of the pus and mucus in sputum: it may be estimated, by attending to the properties of each, as above stated.

Such a compound as the present scarcely is produced in any other part, but in the bronchial and mucous membrane of the nose, because of the abundant secretion of mucus from these membranes. And when it is conceived, that both pus and mucus are secreted in a limpid state, from the same or at least contiguous organs, where they first intimately commix, and then become inspissated; it will appear reasonable, that they cannot be readily, or at all completely separated again from one another. There is indeed, in these cases no necessity for the admission of the secretion of the limpid fluid of pus of abscesses (Sect. VII. 1.); for it appears to me not unjust to consider mucus to be nothing more than the serum of blood, altered in its composition and proportion of water, so as to produce a viscid texture. The secretory organs of the mucous membrane, by virtue of their peculiar power, separate from the blood, in health, the mucus as above said, with some globules, and also a small proportion of the self-coagulable lymph; which appears, on agitating mucus in a large proportion of cold water, in the form of leafy and fibrous masses.* The same secretory organs, it is easily conceivable, may, in a diseased state, be excited to separate also self-coagulable matter from the blood, with more globules, in such a state as to become pus. Hence, such a commixture of the two substances must correspond to the opake, viscid, expectorated sputum, of which I am writing.

If I thought farther reasoning proper, it would be manifest, that all the phenomena, both in health and disease, belonging to the various kinds of sputum, consist with the theory above delivered.

* Serum of blood appears always to contain self-coagulable lymph, which is deposited on standing; and this appearance led Gaber, Pringle, and Cullen, into the erroneous opinion of this deposit being pus itself.

Experiments and Observations on the different Modes in which Death is produced by certain vegetable Poisons. By B. C. Brodie, Esq. F. R. S. Communicated by the Society for promoting the Knowledge of Animal Chemistry.

[From the Philosophical Magazine for August 1811.]

1. THE following experiments were instituted with a view to ascertain, in what manner certain substances act on the animal system, so as to occasion death, independently of mechanical injury. I was led to the inquiry, from the subject of it appearing to be of considerable interest and importance, and from a hope, that, in the present improved state of physiological knowledge, we might be enabled to arrive at some more satisfactory conclusions than had been deduced from any former observations.

The substances which act as poisons when applied to the animal body are very numerous. In the experiments which I have hitherto made, I have employed vegetable poisons only. Of these I have selected such as are very active and certain in producing their effects, believing that, on this account, the exact nature of those effects would be more readily ascertained. The principal objects which I have kept in view have been to determine, on which of the vital organs the poison employed exercises its primary influence, and through what medium that organ becomes affected. I have also endeavoured to ascertain by what means the fatal consequences of some poisons may be prevented. With some of the conclusions which I have ventured to draw, so far as I know, we were not before acquainted; and others of them, though not entirely new, had not been previously established by satisfactory experiments.

I shall relate first those experiments in which poisons were applied internally, that is, to the mucous membranes of the tongue or alimentary canal, and afterwards those in which poisons were applied to wounded surfaces.

II. *Experiments with Poisons applied to the Tongue or alimentary Canal.*

Experiments with Alcohol.

When spirits are taken into the stomach, in a certain quantity, they produce that kind of delirium which constitutes intoxication: when taken in a larger quantity, it is well known that they destroy life altogether, and that in the course of a very short space of time. Intoxication is a derangement of the functions of the mind, and, as these are in some way connected with those of the brain, it seems probable, that it is by acting on this organ, that spirits when taken into the stomach occasion death. In order to ascertain how far this conclusion is just, I made the following experiments.*

Experiment 1. I poured two drachms of proof spirits down the œsophagus of a cat. Instantly he struggled violently; then lay on one side, perfectly motionless and insensible; the breathing was laboured and stertorous, and the pulsations of the heart were very frequent. He continued in this state for seven or eight minutes; then began to recover; the respirations became easier, and presently he stood up, and was able to walk.

Experiment 2. I injected an ounce and a half of proof spirits into the stomach of a large full-grown rabbit, by means of an elastic gum tube passed down the œsophagus. The same symptoms took place as in the last experiment; but the animal did not begin to recover from the state of insensibility until forty minutes had elapsed from the time of the injection.

Experiment 3. Seven drachms of proof spirits were injected into the stomach of a younger rabbit. Two minutes afterwards, he evidently was affected by the spirits, and in three minutes more he lay on one side motionless and insensible. The pupils of the eyes were perfectly dilated; there were occasional slight convulsive motions of the extremities; the respira-

* I am indebted to Dr E. N. Bancroft for his assistance in many of the experiments which I am about to detail. Mr. W. Brande lent me his assistance in the greater part of those which were made. I have been farther assisted by Mr. Broughton, Mr. R. Rawlins, and Mr. R. Gatcombe, and by several other gentlemen.

tion was laborious, it was gradually performed at longer and longer intervals, and at the end of an hour and fifteen minutes had entirely ceased. Two minutes after the animal was apparently dead, I opened into the thorax, and found the heart acting with moderate force and frequency, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration by inflating the lungs, and found that by these means the action of the heart might be kept up to the natural standard, as in an animal from whom the head is removed.

Experiment 4. I injected into the stomach of a rabbit two ounces of proof spirits. The injection was scarcely completed, when the animal became perfectly insensible. Precisely the same symptoms took place as in the last experiment, and at the end of twenty-seven minutes, from the time of the injection, the rabbit was apparently dead; but on examining the thorax the heart was found still acting, as in the last experiment.

It has been shown by M. Bichat, and the observation has been confirmed by some experiments which I have lately had the honour of communicating to this learned Society, that the brain is not directly necessary to the action of the heart, and that, when the functions of the brain are destroyed, the heart continues to contract for some time afterwards, and then ceases only in consequence of the suspension of respiration, which is under the influence of the brain.

It would appear, from the experiments which I have just detailed, that the symptoms produced by a large quantity of spirits taken into the stomach, arise entirely from disturbance of the functions of the brain. The complete insensibility to external impressions; the dilatation of the pupils of the eyes; and the loss of motion, indicate that the functions of this organ are suspended; respiration, which is under its influence, is ill performed, and at last altogether ceases; while the heart, to the action of which the brain is not directly necessary, continues to contract, circulating dark-coloured blood for some time afterwards.

There is a striking analogy between the symptoms arising from spirits taken internally, and those produced by injuries of the brain.

Concussion of the brain, which may be considered as the slightest degree of injury, occasions a state of mind resembling intoxication, and the resemblance in some instances is so complete, that the most accurate observer cannot form a diagnosis, except from the history of the case. Pressure on the brain, which is a more severe injury than concussion, produces loss of motion, insensibility, dilatation of the pupils; respiration becomes laboured and stertorous, is performed at long intervals, and at last altogether ceases, and the patient dies.

It forms an interesting matter of inquiry, whether spirits when taken into the stomach produce their effects on the brain, by being absorbed into the circulation, or in consequence of the sympathy that exists between these organs by means of the nerves. The following circumstances lead me to conclude that they act in the last of these two ways.

1. In experiments where animals have been killed by the injection of spirits into the stomach, I have found this organ to bear the marks of great inflammation, but never found any preternatural appearances whatever in the brain. 2. The effects of spirits taken into the stomach in the last experiment were so instantaneous, that it appears impossible that absorption should have taken place before they were produced. 3. A person who is intoxicated, frequently becomes suddenly sober after vomiting. 4. In the experiments which I have just related, I mixed tincture of rhubarb with the spirits, knowing from the experiments of Mr. Home, and Mr. William Brande, that this, when absorbed into the circulation, was readily separated from the blood by the kidneys, and that very small quantities might be detected in the urine by the addition of potash; but, though I never failed to find urine in the bladder, I never detected rhubarb in it.

The including the termination of the thoracic duct in a ligature does not prevent spirits, when taken into the stomach, from producing their usual effects on the nervous system; but subsequent observations, which Mr. Home has already communicated to this Society, have shown that no conclusion can be drawn from this experiment.

That a poison may affect a distant organ, through the medium of the nerves, without entering the circulation, is proven by

the well-known circumstance of solution of the extract of *bella-donna*, when applied to the tunica conjunctiva of the eye, occasioning dilatation of the pupil of the same eye, though no other part of the system is affected.

It has been formerly supposed by Dr. Mead and other physiologists, that a poison may produce death by acting on the extremities of the nerves of the stomach and intestines, without being absorbed into the circulation. That it should by these means be capable of affecting the brain is not to be wondered at, when we consider the numerous and various sympathies between this organ and the alimentary canal, evidently independent of any other communication than the nerves.

*Experiments with the Essential Oil of Bitter Almonds.**

Experiment 5. One drop of the essential oil of bitter almonds was applied to the tongue of a young cat. She was instantly seized with violent convulsions; then lay on one side motionless, insensible, breathing in a hurried manner; the respirations became laboured, took place at longer and longer intervals, and at the end of five minutes, from the application of the poison, had entirely ceased, and the animal was apparently dead; but, on opening the thorax, the heart was found acting regularly eighty times in a minute, circulating dark-coloured blood, and it continued to act for six or seven minutes afterwards.

Experiment 6. I injected into the rectum of a cat half an ounce of water, with two drops of the essential oil. In two minutes afterwards, he was affected with symptoms similar to those which occurred in the last experiment, and at the end of five minutes, from the injection of the poison, he was apparently dead. Two minutes after apparent death, the heart was found acting eighty times in a minute. On dissection, no preternatural appearances were found either in the internal membrane of the rectum, or the brain.

* The essential oil of bitter almonds does not appear to differ from the essential oil of laurel. I was furnished with a quantity of it, first by my friend Mr. William Brande, and afterwards by Mr. Cooke of Southampton street.

The symptoms produced by this poison, and the circumstance of the heart continuing to contract after apparent death, lead to the conclusion that it occasions death by disturbing the functions of the brain.

While engaged in these last experiments, I dipped the blunt end of a probe into the essential oil, and applied it to my tongue, meaning to taste it, and having no suspicion that so small a quantity could produce any of its specific effects on the nervous system; but scarcely had I applied it, when I experienced a very remarkable and unpleasant sensation, which I referred chiefly to the epigastric region, but the exact nature of which I cannot describe, because I know nothing precisely similar to it. At the same time there was a sense of weakness in my limbs, as if I had not the command of my muscles, and I thought that I was about to fall. However, these sensations were momentary, and I experienced no inconvenience whatever afterwards.

I afterwards applied a more minute quantity of the essential oil to my tongue several times, without experiencing from it any disagreeable effects; but on applying a larger quantity, I was affected with the same momentary sensations as in the former instance, and there was a recurrence of them in three or four seconds after the first attack had subsided.

From the instantaneousness with which the effects are produced, and from its acting more speedily when applied to the tongue than when injected into the intestine, though the latter presents a better absorbing surface, we may conclude that this poison acts on the brain through the medium of the nerves, without being absorbed into the circulation.

Experiment with the Juice of the Leaves of Aconite.

Experiment 7. An ounce of this juice was injected into the rectum of a cat. Three minutes afterwards he voided what appeared to be nearly the whole of the injection; he then stood for some minutes perfectly motionless, with his legs drawn together; at the end of nine minutes, from the time of the injection, he retched and vomited; then attempted to walk, but faltered and fell at every step, as if from giddiness. At the end of thirteen minutes, he lay on one side insensible,

motionless, except some slight convulsive motions of the limbs. The respiration became slow and laboured; and at forty-seven minutes from the time of the injection, he was apparently dead. One minute and a half afterwards, the heart was found contracting regularly one hundred times in a minute.

It appears from this experiment, that the juice of aconite, when injected into the intestine, occasions death by destroying the functions of the brain. From the analogy of other poisons, it is rendered probable that it acts on the brain through the medium of the nerves, without being absorbed into the circulation. This opinion is confirmed by the following circumstance: if a small quantity of the leaf of aconite is chewed, it occasions a remarkable sense of numbness of the lips and gums, which does not subside for two or three hours.

Experiments with the Infusion of Tobacco.

Experiment 8. Four ounces of infusion of tobacco were injected into the rectum of a dog. Four minutes afterwards he retched, but did not vomit; he then became faint, and lay motionless on one side; at the end of nine minutes from the time of the injection, the heart could not be felt; he gasped for breath at long intervals; and in another minute there was no appearance whatever of life. I immediately laid open the cavities of the thorax and abdomen. The heart was much distended, and had entirely ceased to contract; there was no peristaltic motion of the intestines.

Experiment 9. An ounce of very strong infusion of tobacco was injected into the rectum of a cat. Symptoms were produced similar to those which occurred in the last experiment, and the animal died at the end of seven minutes from the time of the injection. On opening the thorax immediately after death, the heart was found extremely distended, and to have entirely ceased acting, with the exception of a slight tremulous motion of the auricles.

Experiment 10. Three ounces of infusion of tobacco were injected into the rectum of a dog. He was affected with symptoms similar to those in the former experiments, and died at the end of ten minutes. On opening the thorax immediately

after death, I found the heart much distended, and to have entirely ceased contracting.

EXPERIMENT 11. Three ounces of infusion of tobacco were injected into the rectum of a dog. Immediately there took place tremulous contractions of the voluntary muscles. Five minutes afterwards the injection was repeated in the same quantity. The dog then was sick, and threw up some of the infusion, with other matter, from the stomach; he became faint, and died ten minutes after the second injection. Immediately after respiration had ceased, I opened the thorax, and found the heart extremely distended, and without any evident contraction, except of the appendix of the right auricle, which every now and then contracted in a slight degree. I divided the pericardium on the right side. In consequence of the extreme distention of the heart, this could not be done without irritating the fibres with the point of the scalpel. Immediately both auricles and ventricles began to contract with considerable force, so as to restore the circulation. Artificial respiration was produced, and the circulation was kept up for more than half an hour, beyond which time the experiment was not continued.

We may conclude from these experiments, that the effect of the infusion of tobacco, when injected into the intestine of a living animal, is to destroy the action of the heart, stopping the circulation and producing syncope. It appeared to me that the action of the heart ceased even before the animal had ceased to respire; and this was confirmed by another experiment, in which, in a dog killed by the infusion of tobacco, I found the cavities of the left side of the heart to contain scarlet blood, while in those of the right side the blood was dark-coloured. This poison therefore differs materially from alcohol, the essential oil of almonds, and the juice of aconite, which have no direct influence on the action of the heart. The infusion of tobacco renders the heart insensible to the stimulus of the blood, but it does not altogether destroy the power of muscular contraction, since the heart resumed its action in one instance on the division of the pericardium, and I have found that the voluntary muscles of an animal killed by this poison, are as readily stimulated to contract by the influence of the voltaic battery, as if it had been killed in any other manner.

At the same time, however, that the infusion of tobacco destroys the action of the heart, it appears to destroy also the functions of the brain, since these did not return in the last experiment; although the circulation was restored, and kept up by artificial respiration.

Since there is no direct communication between the intestinal canal and the heart, I was at first induced to suppose that the latter becomes affected in consequence of the infusion being conveyed into the blood by absorption. Some circumstances in the following experiment have since led me to doubt whether this is the case.

Experiment 12. In a dog, whose head was removed, I kept up the circulation by means of artificial respiration, in the manner already described in the account of some experiments which I lately communicated to this society. I then injected into the stomach and intestines nine ounces of infusion of tobacco. At the time of the injection, the body of the animal lay perfectly quiet and motionless on the table; the heart acted regularly one hundred times in a minute. Ten minutes afterwards the pulse rose to one hundred and forty in a minute; the peristaltic motion of the intestines was much increased, and the voluntary muscles in every part of the body were thrown into repeated and violent spasmodic action. The joints of the extremities were alternately bent and extended; the muscles of the spine, abdomen, and tail alternately relaxed and contracted, so as to turn the whole animal from one side to the other. I have observed, in other instances, spasmodic actions of the muscles, where the circulation was kept up by artificial respiration, after the removal of the head, but not at all to be compared, either in strength or frequency, with those which took place on this occasion. I made pressure on the abdominal aorta for more than a minute, so as to obstruct the circulation of the blood in the lower extremities; but the muscular contractions were not lessened in consequence. Half an hour after the injection of the infusion, the artificial respiration was discontinued. The heart continued to act, circulating dark-coloured blood; the muscular contractions continued, but gradually diminished in strength and frequency. I tied a ligature round the vessels at the base of the heart, so as to stop the cir-

culation; nevertheless, the muscular contractions still continued, though less frequent and forcible than before, and some minutes elapsed before they entirely ceased.

In this experiment, the disposition to contraction in the muscles was very much increased, instead of being diminished, as in those just related. If the infusion of tobacco influences the heart, from being absorbed into the blood and thus coming into actual contact with its fibres, there is no evident reason why the removal of the brain, and the employment of artificial respiration, should occasion so material a difference in its effects. If the contractions of the voluntary muscles had depended on the infusion circulating with the blood, it is reasonable to suppose that the pressure on the aorta would have occasioned some diminution of them, and that the complete obstruction of the circulation would have caused them to cease altogether.

From these considerations, I am induced, on the whole, to believe that the infusion of tobacco, when injected into the intestines, influences the heart through the medium of the nervous system; but I have not been able to devise any experiment, by which the truth or fallacy of this opinion might be put beyond the reach of doubt.

It appears remarkable, that the brain and nervous system, although not necessary to the action of the heart, should, when under the influence of the infusion of tobacco, be capable of influencing this organ so as to stop its action; but this is analogous to what we see occur in consequence of violent emotions of the mind. Those states of the nervous system, which accompany the passions of joy, fear, or anger, when existing in a moderate degree, render the heart more sensible to the stimulus of the blood, and increase the frequency of its contractions; while, when the same passions exist in a greater degree, the heart is rendered altogether insensible to the stimulus of the blood, and syncope ensues.

*Experiments with the Empyreumatic Oil of Tobacco.**

Experiment 13. Less than a drop of this oil was applied

* I was furnished with the empyreumatic oil of tobacco by Mr. W. Brande. It may be procured by subjecting the leaves of tobacco to distillation in a

to the tongue of a young cat. Instantly violent convulsions took place in all the muscles, and the respirations became very frequent. In five minutes after the application, she lay on one side insensible, with slight spasmodic actions of the muscles. At the end of eleven minutes she retched, but did not vomit. In a quarter of an hour she appeared to be recovering. I repeated the application of the poison, and she was again seized with violent convulsions, and became insensible, breathing at long intervals; and in two minutes from the second application respiration had entirely ceased, and she was apparently dead. On opening the thorax, I found the heart acting with regularity and strength, circulating dark-coloured blood. I introduced a tube into the trachea, and produced artificial respiration; the contractions of the heart became augmented in force and frequency, and there was no evident diminution in six or seven minutes, during which the artificial respiration was continued.

On dissection, nothing remarkable was found in the appearance of the tongue or brain.

The symptoms and mode of death, in this experiment, did not essentially differ from those produced by the essential oil of almonds. I was surprised to find the effects of the empyreumatic oil so entirely different from those of the infusion of tobacco. Supposing that this difference might arise from the poison being more concentrated in the oil than in the infusion, I made the following experiments.

Experiment 14. A drop of the oil of tobacco was suspended in an ounce and a half of water by means of mucilage of gum arabic, and the whole was injected into the rectum of a dog. In two minutes afterwards he became faint, retched, but did not vomit. He appeared to be recovering from this state, and in twenty-five minutes after the first injection, it was repeated in the same quantity. He was then seized with symptoms similar to those in the last experiment, and in two minutes and a half he was apparently dead.

Two minutes after apparent death, on the thorax being opened into, the heart was found acting regularly one hundred times in a minute, and it continued acting for several minutes.

heat above that of boiling water: a quantity of watery fluid comes over, on the surface of which is a thin film of unctuous substance.

Experiment 15. A drop of the empyreumatic oil of tobacco with an ounce of water was injected into the rectum of a cat. The symptoms produced were in essential circumstances similar to those which occurred in the last experiment. The animal was apparently dead in five minutes after the injection, and the heart continued to contract for several minutes afterwards.

We may conclude from these experiments, that the empyreumatic oil of tobacco, whether applied to the tongue or injected into the intestine, does not stop the action of the heart and induce syncope, like the infusion of tobacco; but that it occasions death by destroying the functions of the brain, without directly acting on the circulation. In other words, its effects are similar to those of alcohol, the juice of aconite, and the essential oil of almonds.

III. *Experiments with Poisons applied to wounded Surfaces.*

Experiments with the Essential Oil of Almonds.

Experiment 16. I made an incision in the thigh of a rabbit, and introduced two drops of essential oil between the skin and the muscles. In four minutes after the application, he was seized with violent convulsions, and became insensible, and in two minutes more he was apparently dead; but the heart was felt through the ribs acting one hundred and twenty times in a minute, and it continued acting for several minutes. There were no other appearances in the limb, than would have resulted from an ordinary wound.

Experiment 17. Two drops of the essential oil of almonds were introduced into a wound in the side of a mouse. Two minutes afterwards he was affected with symptoms similar to those which occurred in the last experiment, and in two minutes more he was apparently dead, but the heart continued to contract for some minutes afterwards.

From the experiments which I have just related, and from others which it appears unnecessary to detail, as the general results were the same, I have learned that where the essential oil of almonds is applied to a wound, its effects are not so instantaneous as when it is applied to the tongue; otherwise

there is no difference in its effects, in whatever manner it is applied.

Experiments with the Juice of the Leaves of Aconite.

EXPERIMENT 18. I made a wound in the side of a young rabbit, and introduced, between the skin and muscles, about twenty drops of the juice of aconite. Twenty-three minutes afterwards he was affected with symptoms in all essential respects similar to those which occurred in an experiment already related, where the juice was injected into the rectum, and at the end of forty-seven minutes from the application of the poison he was apparently dead. Two minutes after apparent death, the heart was found contracting, but very feebly.

*Experiments with the Woorara.**

EXPERIMENT 19. A small quantity of the woorara in powder was applied to a wound in the side of a Guinea pig. Ten minutes afterwards the animal was unable to walk; then he became quite motionless, except some slight occasional convulsions. He gradually became insensible; the respirations were laboured, and at the end of fourteen minutes from the application of the poison the respiration had entirely ceased, and he was apparently dead; but on opening the thorax, the heart was found acting seventy times in a minute, circulating dark-coloured blood, and it continued to contract for several minutes afterwards. On dissection, no preternatural appearances were observed in the brain, nor was there any other appearance in the limb than would have arisen from an ordinary wound.

EXPERIMENT 20. I made a wound in the side of a Guinea pig, and introduced into it about two grains of the woorara in powder. At the end of twenty-five minutes, symptoms took place very similar to those which occurred in the last experiment, and in thirteen minutes more the animal was apparently dead; but the heart continued to contract one hundred and

* The Woorara is a poison with which the Indians of Guyana arm the points of their arrows. It appears not to differ essentially from the Ticunas, which was employed in the experiments of the Abbé Fontana. I am indebted to Dr. E. N. Bancroft, who not only furnished me with some of the woorara which he had in his possession, but also lent me his assistance in the experiments which were made with it.

eight times in a minute, and by means of artificial respiration the circulation was kept up for more than twenty minutes.

The results of other experiments which I have made with the woorara were similar to those just described. The heart continued to act after apparent death, and the circulation might be kept up by means of artificial respiration. It is evident that this poison acts in some way or other on the brain, and that the cessation of the functions of this organ is the immediate cause of death.

I found in these experiments, that the best mode of applying the woorara is when it is dissolved in water to the consistence of a thin paste. I first made the wound, and then smeared the poison over it with the end of the scalpel. I found that the animal was more speedily and certainly affected, if there was some hemorrhage, unless the hemorrhage was very copious, when it produced an opposite effect, by washing the poison away from the wound. When the poison was applied in large quantity, it sometimes began to act in six or seven minutes. Never more than half an hour elapsed from the time of the poison being inserted, to that of the animal being affected, except in one instance, where a ligature was applied on the limb, which will be mentioned afterwards. The woorara, which I employed, had been preserved for some years, which will account for its having been less active than it has been described to be by those who had witnessed its effects when in a recent state.

*Experiments with the Upas Antiar.**

Experiment 21. About two grains of this poison were made into a thin paste with water, and inserted into a wound in the thigh of a dog. Twelve minutes afterwards he became languid; at the end of fifteen minutes, the heart was found to beat very irregularly, and with frequent intermissions; after this, he had a slight rigor. At the end of twenty minutes, the heart

* We are informed that the island of Java produces two powerful vegetable poisons, to one of which the natives give the name of *Upas tieuté*, and to the other that of *Upas antiar*. I was supplied with a quantity of the latter through the kindness of Mr. Marsden, who had some of it in his possession.

beat very feebly and irregularly; he was languid; was sick and vomited; but the respirations were as frequent and as full as under natural circumstances, and he was perfectly sensible. At the end of twenty minutes he suddenly fell on one side, and was apparently dead. I immediately opened into the thorax, and found the heart distended with blood in a very remarkable degree, and to have entirely ceased contracting. There was one distinct and full inspiration after I had begun making the incision into the thorax. The cavities of the left side of the heart contained scarlet blood, and those of the right side contained dark-coloured blood, as in a living animal.

EXPERIMENT. 22. A small quantity of the upas antiar, prepared as before, was inserted into a wound in the thigh of a young cat. She appeared languid in two minutes after the poison was inserted. The symptoms which took place did not essentially differ from those which occurred in the last experiment, except that there were some convulsive motions of the limbs. At eight minutes after the poison was inserted, she lay on one side motionless and insensible, the heart could not be felt, but the respiration had not entirely ceased. On opening into the thorax, I found the heart to have ceased contracting. It was much distended with blood: and the blood in the cavities of the left side was of a scarlet colour. There were two full inspirations after the incision of the thorax was begun. On irritating the heart with the point of the scalpel, slight contractions took place in the fibres of the appendices of the auricles, but none in any other part.

EXPERIMENT 23. The experiment was repeated on a rabbit. The symptoms produced were similar to those in the last experiment; but the animal did not vomit, and the convulsive motions were in a less degree: he died eleven minutes after the poison was inserted. On opening the chest, the heart was found to have entirely ceased contracting; it was much distended with blood; and the blood in the cavities of the left side was of a scarlet colour. On irritating the heart with the point of the scalpel, the ventricles contracted, but not sufficiently to restore the circulation.

EXPERIMENT 24. About a grain of the upas antiar was inserted into a wound in the side of a rabbit. He was affected

with symptoms similar to those before described, and died in ten minutes after the poison was applied. On opening the thorax immediately after death, the heart was found to have ceased contracting, and the blood in the cavities of the left side was of a scarlet colour.

It appears from these experiments, that the upas antiar, when inserted into a wound, produces death (as infusion of tobacco does when injected into the intestines) by rendering the heart insensible to the stimulus of the blood, and stopping the circulation. The heart beats feebly and irregularly before either the functions of the mind or the respiration appear to be affected. Respiration is performed even after the circulation has ceased; and the left side of the heart is found after death to contain scarlet blood, which never can be the case where the cause of death is the cessation of the functions of the brain or lungs. The convulsions which occur when the circulation has nearly ceased, probably arise from the diminution of the supply of blood to the brain, resembling those which take place in a person who is dying from hemorrhage.

There remains an interesting subject of inquiry, "through what medium do poisons influence the brain when applied to wounds?" That poisons applied in this manner do not produce their effects precisely in the same way as poisons taken internally, is rendered probable by this circumstance; that some poisons, which are very powerful when applied to wounds even in small quantities, are either altogether inefficient when taken internally, or require to be given in very large quantities, in order to produce their effect, and *vice versâ*.

A poison applied to a wounded surface may be supposed to act on the brain in one of three ways,

1. By means of the nerves, like poisons taken internally.

2. By passing into the circulation through the absorbent vessels.

3. By passing directly into the circulation through the divided veins.

Experiment 25. In order to ascertain whether the woorara acts through the medium of the nerves, I exposed the axilla of a rabbit, and divided the spinal nerves supplying the upper extremity, just before they unite to form the axillary plexus.

The operation was performed with the greatest care. I not only divided every nervous filament, however small, which I could detect, but every portion of cellular membrane in the axilla, so that the artery and vein were left entirely insulated. I then made two wounds in the fore-arm, and inserted into them some of the woorara formed into a paste. Fourteen minutes after the poison was applied, the hind legs became paralytic, and in ten minutes more he died, with symptoms precisely similar to those which took place in the former experiments, and the heart continued to act after apparent death. On dissection, the nerves of the upper extremity were particularly examined, but not the smallest filament could be found undivided.

I made the following experiment, to ascertain whether the woorara passes into the circulation through the absorbent vessels.

Experiment 26. I tied a ligature round the thoracic duct of a dog, just before it perforates the angle of the left subclavian and jugular veins. I then made two wounds in the left hind leg, and introduced some of the woorara in powder into them. In less than a quarter of an hour he became affected with the usual symptoms, and died in a few minutes afterwards.

After death, I dissected the thoracic duct with great care. I found it to have been perfectly secured by the ligature. It was very much distended with chyle; and about two inches below its termination its coats had given way, and chyle was extravasated into the cellular membrane. The lymphatic vessels in the left axilla were distended in a very remarkable degree; and on dividing them, not less than a drachm of lymph issued from the divided ends.

Since neither the division of the nerves nor the obstruction of the thoracic duct interfere in the slightest degree with the effects of the woorara, there is presumptive evidence that it acts on the brain by entering the circulation through the divided veins. I endeavoured to ascertain, by experiment, whether this is really the case.

To apply ligatures to the large vessels of a limb only would evidently lead to no satisfactory conclusion, since the anastomosing vessels might still carry on the circulation. The only

way which I could devise of performing the experiment, was to include all the vessels, small as well as large, in a ligature.

Experiment 27. In order to make the experiment more satisfactorily, I exposed the sciatic nerve of a rabbit in the upper and posterior part of the thigh, and passed under it a tape half an inch wide. I then made a wound in the leg, and having introduced into it some of the woorara mixed with water, I tied the tape moderately tight on the forepart of the thigh. Thus I interrupted the communication between the wounds and the other parts of the body, by means of the vessels, while that by means of the nerve still remained. After the ligature was tightened, I applied the woorara a second time, in another part of the leg. The rabbit was not at all affected, and at the end of an hour I removed the ligature. Being engaged in some other pursuit, I did not watch the animal so closely as I should otherwise have done; but twenty minutes after the ligature was removed, I found him lying on one side, motionless and insensible, evidently under the influence of the poison; but the symptoms were less violent than in most instances, and after lying in this state he recovered, and the limb became perfectly warm, and he regained the power of using it.

Experiment 28. I repeated the last experiment with this difference, that after having applied the poison, I made the ligature as tight as I could draw it. I removed the ligature at the end of an hour and twenty minutes, but the animal was not at all affected either before or after the removal of the ligature, and on the following day he had recovered the use of the limb.

Experiment 29. I repeated the experiment a third time, drawing the ligature very tight. At the end of forty-five minutes the animal continued perfectly well, and the ligature was removed. I watched him for three quarters of an hour afterwards, but there were no symptoms of his being affected by the poison. On the following day the rabbit died; but this I attribute to the injury done to the limb and sciatic nerve by the ligature, as there was the appearance of inflammation in the parts in the neighbourhood of the ligature.

These three experiments were made with the greatest care. From the mode in which the poison was applied, from the quantity employed, and from my prior experience, I should

have entertained not the smallest doubt of the poison taking effect in every instance in less than twenty minutes, if no ligature had been applied. In two of the three, the quantity of woorara was more than had been used in any former experiments.

I have not judged it necessary to make any more experiments, with the ligature on the limb, because the numerous experiments of the Abbé Fontana on the ticunas, coincide in their results with those which have just been detailed, and fully establish the efficacy of the ligature in preventing the action of the poison. It is not to be wondered at, that the ligature should sometimes fail in its effects, since these must evidently depend on the degree in which the circulation is obstructed, and on the length of time during which the obstruction is continued.

There can be little doubt that the woorara affects the brain, by passing into the circulation through the divided vessels. It is probable that it does not produce its effects, until it enters the substance of the brain, along with the blood, in which it is dissolved; nor will the experiments of the Abbé Fontana, in which he found the ticunas produce almost instant death when injected into the jugular vein of a rabbit, be found to militate against this conclusion, when we consider how short is the distance which, in so small an animal, the blood has to pass from the jugular vein to the carotid artery, and the great rapidity of the circulation; since in a rabbit under the influence of terror, during such an experiment, the heart cannot be supposed to act so seldom as three times in a second.

I have made no experiments to ascertain through what medium other poisons when applied to wounds affect the vital organs, but from analogy we may suppose that they enter the circulation through the divided bloodvessels.

IV.

The facts already related led me to conclude that alcohol, the essential oil of almonds, the juice of aconite, the oil of tobacco, and the woorara, occasion death simply by destroying the functions of the brain. The following experiment appears fully to establish the truth of this conclusion,

EXPERIMENT 30. The temperature of the room being 58° of Fahrenheit's thermometer, I made two wounds in the side of a rabbit, and applied to them some of the woorara in the form of paste. In seven minutes after the application, the hind legs were paralized, and in fifteen minutes respiration had ceased, and he was apparently dead. Two minutes afterwards the heart was still beating, and a tube was introduced through an opening into the trachea, by means of which the lungs were inflated. The artificial respiration was made regularly about thirty-six times in a minute.

At first, the heart contracted one hundred times in a minute.

At the end of forty minutes, the pulse had risen to one hundred and twenty in a minute.

At the end of an hour, it had risen to one hundred and forty in a minute.

At the end of an hour and twenty-three minutes, the pulse had fallen to a hundred, and the artificial respiration was discontinued.

At the commencement of the experiment, the ball of a thermometer being placed in the rectum, the quicksilver rose to one hundred degrees; at the close of the experiment it had fallen to eighty-eight and a half.

During the continuance of the artificial respiration, the blood in the femoral artery was of a florid red, and that in the femoral vein of a dark colour, as usual.

It has been observed by M. Bichat, that the immediate cause of death, when it takes place suddenly, must be the cessation of the functions of the heart, the brain, or the lungs. This observation may be extended to death under all circumstances. The stomach, the liver, the kidneys, and many other organs are necessary to life, but their constant action is not necessary; and the cessation of their functions cannot therefore be the *immediate* cause of death. As in this case the action of the heart had never ceased; as the circulation of the blood was kept up by artificial respiration for more than an hour and twenty minutes after the poison had produced its full effects; and as during this time the usual changes in the colour of the blood took place in the lungs; it is evident that the functions of the heart and lungs were unimpaired: but that those of the

brain had ceased, is proved by the animal having continued in a state of complete insensibility; and by this circumstance, that animal heat, to the generation of which I have formerly shown the influence of the brain to be necessary, was not generated.

Having learned that the circulation might be kept up by artificial respiration for a considerable time after the woorara had produced its full effects, it occurred to me that in an animal under the influence of this or of any other poison that acts in a similar manner, by continuing the artificial respiration for a sufficient length of time after natural respiration had ceased, the brain might recover from the impression which the poison had produced, and the animal might be restored to life. In the last experiment, the animal gave no sign of returning sensibility; but it is to be observed, 1. That the quantity of the poison employed was very large. 2. That there was a great loss of animal heat, in consequence of the temperature of the room being much below the natural temperature of the animal, which could not therefore be considered under such favourable circumstances as to recovery, as if it had been kept in a higher temperature. 3. That the circulation was still vigorous when I left off inflating the lungs, and therefore it cannot be known what would have been the result, if the artificial respiration had been longer continued.

Experiment 31. A wound was made in the side of a rabbit, and one drop of the essential oil of almonds was inserted into it, and immediately the animal was placed in a temperature of 90°. In two minutes he was under the influence of the poison. The usual symptoms took place, and in three minutes more respiration had ceased, and he lay apparently dead, but the heart was still felt beating through the ribs. A tube was then introduced into one of the nostrils, and the lungs were inflated about thirty-five times in a minute. Six minutes after the commencement of artificial respiration, he moved his head and legs, and made an effort to breathe. He then was seized with convulsions, and again lay motionless, but continued to make occasional efforts to breathe. Sixteen minutes after its commencement, the artificial respiration was discontinued. He now breathed spontaneously seventy times in a minute, and moved his head and extremities. After this, he occasionally

rose, and attempted to walk. In the intervals he continued in a dozing state; but from this he gradually recovered. In less than two hours he appeared perfectly well, and he continued well on the following day.

The inflating the lungs has been frequently recommended in cases of suffocation, where the cause of death is the cessation of the functions of the lungs: as far as I know, it has not been before proposed in those cases, in which the cause of death is the cessation of the functions of the brain.* It is probable that this method of treatment might be employed with advantage for the recovery of persons labouring under the effects of opium, and many other poisons.

V.

The experiments which have been detailed lead to the following conclusions.

1. Alcohol, the essential oil of almonds, the juice of aconite, the empyreumatic oil of tobacco, and the woorara, act as poisons by simply destroying the functions of the brain; universal death taking place, because respiration is under the influence of the brain, and ceases when its functions are destroyed.

2. The infusion of tobacco when injected into the intestine, and the upas antiar when applied to a wound, have the power of rendering the heart insensible to the stimulus of the blood, thus stopping the circulation; in other words, they occasion syncope.

3. There is reason to believe that the poisons, which in these experiments were applied internally, produce their effects through the medium of the nerves without being absorbed into the circulation.

* Since this paper was read, I have been favoured by the right honourable the president with the perusal of a Dissertation on the Effects of the Upas Tieutè, lately published at Paris by M. Delile, by which I find that he had employed artificial respiration for the purpose of recovering animals, which were under the influence of this poison, with success. M. Delile describes the Upas Tieutè as causing death, by occasioning repeated and long-continued contractions of the muscles of respiration, on which it acts through the medium of the spinal marrow, without destroying the functions of the brain.

4. When the woorara is applied to a wound, it produces its effects on the brain, by entering the circulation through the divided bloodvessels, and, from analogy, we may conclude that other poisons, when applied to wounds, operate in a similar manner.

5. When an animal is apparently dead from the influence of a poison, which acts by simply destroying the functions of the brain, it may, in some instances at least, be made to recover, if respiration is artificially produced, and continued for a certain length of time.

From analogy we might draw some conclusions respecting the mode in which some other vegetable poisons produce their effects on the animal system; but I forbear to enter into any speculative inquiries; as it is my wish, in the present communication, to record such facts only, as appear to be established by actual experiment.

Addition to the Croonian Lecture for the year 1810.

In the experiments formerly detailed, where the circulation was maintained by means of artificial respiration after the head was removed, I observed that the blood, in its passage through the lungs, was altered from a dark to a scarlet colour, and hence I was led to conclude that the action of the air produced in it changes analogous to those which occur under ordinary circumstances. I have lately, with the assistance of my friend Mr. W. Brande, made the following experiment, which appears to confirm the truth of this conclusion.

An elastic gum bottle, having a tube and a stop-cock connected with it, was filled with about a pint of oxygen gas. The spinal marrow was divided in the neck of a young rabbit, and the bloodvessels having been secured, the head was removed, and the circulation was maintained by inflating the lungs with atmospheric air for five minutes, at the end of which time the tube of the gum bottle was inserted into the trachea, and carefully secured by a ligature, so that no air might escape. By making pressure on the gum bottle, the gas was made to pass and repass into and from the lungs about thirty times in a minute. At first, the heart acted one hundred and twenty times in a minute, with regularity and strength; the thermo-

meter, in the rectum, rose to 100°. At the end of an hour, the heart acted as frequently as before, but more feebly; the blood in the arteries was very little more florid than that in the veins; the thermometer in the rectum had fallen to 93°. The gum bottle was then removed. On causing a stream of the gas which it contained to pass through lime-water, the presence of carbonic acid was indicated by the liquid being instantly rendered turbid. The proportion of carbonic acid was not accurately determined; but it appeared to form about one half of the quantity of gas in the bottle.

B. C. BRODIE.

Report of the National Vaccine Establishment.

[From the Philosophical Magazine, for August, 1811.]

The board of the national vaccine establishment having learned that great interest has been excited in the public mind, by the occurrence of smallpox after vaccination, in the families of the earl of Grosvenor and of sir Henry Martin, Bart. have thought it their duty to lay the following cases before the public, accompanied with some observations, and a statement how far, in their opinion, these cases affected the general advantages of vaccination.

The case of the honourable Robert Grosvenor, third son of the earl of Grosvenor, was procured through the favour of sir Henry Halford and sir Walter Farquhar, the physicians who attended the young gentleman during his illness; and the case of the son of sir Henry Martin was obtained through the favour of Dr. Heberden. Both of these cases were also visited by the director of the vaccine establishment.

I. *The Case of the Hon. Robert Grosvenor.*

On Sunday, May 26, 1811, the honourable Robert Grosvenor, who was recovering from the whooping-cough, became much indisposed, and threw up his dinner. Fever followed, and he complained most particularly of excruciating pain in his back. He dwelt on this symptom until Thursday, when he became delirious, and there were observed on his face about twenty spots.

He had been vaccinated by Dr. Jenner, in his infancy, about ten years ago, and the mark left in his arm indicated a perfect disease.

On Friday morning, the eruption had not increased materially in point of number; but the appearance of the spots, and the previous symptoms, suggested strongly a suspicion that the disorder was the smallpox.

Sir H. Halford had occasion to go to Windsor in the afternoon of Friday, and did not see Mr. Robert Grosvenor until the Monday following (June 2d); but he learned from sir W. Farquhar, who attended him most carefully during sir Henry's absence, (and subsequently), that the eruption had increased prodigiously in the course of Friday; that on the evening of that day Mr. Robert Grosvenor began to make bloody water, and that he continued to do so until Monday morning.

On the tenth day of the disease the pustules began to dry upon the face, which was swollen to a considerable degree, but not to the extent of closing his eyes, and was attended by a salivation, which lasted several days. Petechiæ had occurred in the interstices of several of the spots, particularly on the limbs, and there was that particular smell from the whole frame which is remarkable in bad cases of confluent smallpox.

It was obvious that the first symptoms of which Mr. Grosvenor complained, were such as indicated a violent disease about to follow; and sir Henry confesses that he entertained a most unfavourable opinion of the issue of such a malady, when it was fully formed; having never seen an instance of recovery under so heavy an eruption attended by such circumstances. It seemed, however, that the latter stages of the disease were passed through more rapidly in this case than usual: and it may be a question whether this extraordinary circumstance, as well as the ultimate recovery of Mr. Grosvenor, were not influenced by previous vaccination.

HENRY HALFORD,
WALTER FARQUHAR.

In addition to the preceding account, the board have authority to state, that during the illness of Mr. Grosvenor, the other

children of the earl of Grosvenor, who had been previously vaccinated, were exposed to the contagion of the smallpox under which their brother was suffering, and were also submitted to smallpox inoculation without effect.

II. *The Case of the Son of Sir Henry Martin.*

Sir Henry Martin's son, aged eleven years, was vaccinated by Mr. Tegart in the year 1801, and exhibited all the usual marks of that disorder in a complete and satisfactory manner.

He still retains on his arm the characteristic scar.

This boy was taken ill on Saturday the 22d day of June, 1811; at the period of the attack he was recovering from whooping-cough.

23d. Continued to be feverish.

24th. Mr. Tegart was sent to.

25th. The fever increased, and at night he became delirious.

26th. An eruption was perceived chiefly about the mouth, at the same time his eyes and throat were slightly inflamed. The fever continued.

27th, or 2d day of the eruption, the pustules increased, so as to afford suspicion of the chickenpox.

3d day of the eruption, the pustules increased, the fever decreased.

4th. At the close of the fourth day, Dr. Heberden first saw this boy, with a distinct eruption of the most perfect kind of smallpox, all pretty uniform in size, well filled with a fluid already beginning to grow yellow, and surrounded by a rose-coloured margin precisely like smallpox of the fifth day. There were about one hundred pustules on the face, and perhaps twice as many on the limbs, but the trunk was almost free; the features were swollen, but not very much so. The skin was hot, and the pulse quick.

5th day. The pustules were more uniform, and yellow, and the patient complained of soreness; but he was cooler, and his pulse was quieter.

6th day. The fever had entirely subsided, and the pock began to turn.

8th. The pustules were dried, and continued to fall off from the face. The boy continued quite well.

W. HEBERDEN.

Pall Mall, July 4, 1811.

With a view of obtaining the most accurate knowledge of the early symptoms of this case, which did not come under the immediate observation of Dr. Heberden, the board have procured, through the favour of Mr. Tegart, of Pall Mall, an account of the commencement and course of the disorder, which corroborates the above statement. And from the same source they have been informed, that miss Martin and a nursery maid of sir H. Martin's family, who had both been vaccinated, were inoculated with matter taken from master Martin on the fifth day of the eruption, and were exposed to the contagion of the smallpox during the course of his disorder, without effect.

The board are of opinion, that the case of the honourable Robert Grosvenor was a case of confluent smallpox. That the attack and progress of the disorder were attended by symptoms which almost invariably announce a fatal termination. But they observe, that the swelling of the face, which is generally so excessive as to close the eyes, and is considered as a favourable symptom, was slighter than usual; that on the tenth day the pustules began to dry upon the face; and that from that time the disease passed with unusual rapidity through the period when life is generally esteemed to be in the greatest hazard.

Those who are acquainted with the nature of the confluent smallpox, are aware that this peculiarity cannot be attributed to the effect of medical treatment.

The case of the son of sir Henry Martin exhibits a mild form of distinct smallpox occurring after vaccination.

In most cases of smallpox which have succeeded to vaccination, the pustules have been observed to dry more rapidly, and the disorder has concluded at an earlier period than usual.

If allowance be made for the relative periods in which the confluent and distinct smallpox complete their course, the rapid progress towards recovery through the latter stage of

confluent smallpox, as exhibited in the case of Mr. Grosvenor, may be compared with the rapid desiccation of the pustules in the distinct and peculiarly mild form of the disorder which is considered as smallpox modified by vaccination. Both forms of the disorder proceed in the usual course, the one attended with violent, the other with mild symptoms, till they arrive near to the height; when they appear to receive a check, and the recovery is unusually rapid.

From this correspondence of circumstances, the board are induced to infer that in the case of Mr. Grosvenor, which has been more violent than any yet submitted to them, the progress of the disease, through its latter stage, and the consequent abatement of symptoms, were influenced by an antivariolous effect produced upon the constitution by the vaccine process.

The occurrence of smallpox after vaccination has been foreseen and pointed out in the report on vaccination made to parliament, by the college of physicians, in the year 1807, to which the board are desirous of calling the attention of the public; wherein it is stated that,

"The security derived from vaccination against the smallpox, if not absolutely perfect, is as nearly so as can perhaps be expected from any human discovery; for amongst several hundred thousand cases, with the results of which the college have been made acquainted, the number of alleged failures has been surprisingly small, so much so as to form certainly no reasonable objection to the general adoption of vaccination; for it appears that there are not nearly so many failures in a given number of vaccinated persons, as there are deaths in an equal number of persons inoculated for the smallpox. Nothing can more clearly demonstrate the superiority of vaccination over the inoculation of the smallpox than this consideration; and it is a most important fact, which has been confirmed in the course of this inquiry, that in almost every case in which the smallpox has succeeded vaccination, whether by inoculation or by casual infection, the disease has varied much from its ordinary course; it has neither been the same in violence nor in the duration of its symptoms; but has, with very few exceptions, been remarkably mild, as if the smallpox had been de-

prived by the previous vaccine disease of its usual malignity." Vide report of the college of physicians.

The peculiarities of certain constitutions with regard to eruptive fevers, form a curious subject of medical history. Some individuals have been more than once affected with scarlet fever and measles, others have been through life exposed to the contagion of these diseases without effect; many have resisted the inoculation and contagion of smallpox for several years, and have afterwards become susceptible of the disorder, and some have been twice affected with smallpox.

Among such infinite varieties of temperament it will not appear extraordinary, that vaccination, though so generally successful, should sometimes fail of rendering the human constitution unsusceptible of smallpox, especially since it has been found that in several instances smallpox has occurred to individuals over whom the smallpox inoculation had appeared to have produced its full influence. Three instances of this kind have taken place within the last month, and in another instance the natural smallpox has occurred a second time.

I. *Case of the Rev. Joshua Rowley.*

The reverend Joshua Rowley, brother to sir W. Rowley, when an infant, was inoculated by the late Mr. Adair, 1770; the scar left by the inoculation is perfectly visible; his mother, the dowager lady Rowley, remembers perfectly his having a tolerable sprinkling of smallpox, and says, he was afterwards repeatedly exposed to variolous infection in the nursery, when his three younger brothers were successively inoculated, all of whom had some degree of eruption; and since that time, frequently, in performing the clerical duties of his profession.

On Wednesday the 5th of June, he felt much indisposed, complained of pain in his head and back, attended with considerable restlessness and prostration of strength: on Friday the 7th, an eruption appeared chiefly on his face and breast; he was attended by Mr. Woodman, of Bognor, only, till the Monday following, when Mr. Guy, surgeon, of Chichester was first consulted. On examining the eruption, Mr. Guy was immediately struck with its resemblance to the smallpox; and on gently hinting his suspicion to Mr. Rowley, received the in-

formation above related. On the following day the progress of the eruption towards maturation, and the swelling of the face, which is characteristic of the smallpox, left no doubt of the nature of the malady. The eruption was perfectly distinct; it was very full all over the trunk and body, and there were about two hundred pustules on the face. Mr. Guy is of opinion, that this was a clearly marked case of smallpox.

The history of the previous variolous inoculation in 1770, was procured from the dowager lady Rowley by Mr. Dundas, sergeant surgeon to his majesty; and the account of the present case was transmitted to the director of vaccination of this establishment, on the application of the board, by Mr. Guy, an eminent surgeon of Chichester.

II. *Case of Miss Sarah Booth, of Covent Garden Theatre.*

Dr. Bree was called to visit miss S. Booth, on Monday, June 25th. She was said to be ill with the smallpox; and the following circumstances were reported by the mother and sisters.

Miss Booth is eighteen years of age; she had been inoculated for the smallpox at five years of age, and had been affected with the usual degree of fever; the arm had been violently inflamed, and an eruption of smallpox pustules had appeared round the inoculated part, from which matter had been taken by Mr. Kennedy, the surgeon who attended her. Mr. Kennedy expressed himself satisfied that miss S. Booth had passed regularly through the disease.

The usual scar of smallpox inoculation is perfectly evident on the arm.

On Thursday, June 20th, miss Booth was seized with fever, distinguished by vomiting, violent headache, pains in the back and loins.

The symptoms continued till Saturday, June 22d, in the evening of which day some pustules came out on the forehead and scalp.

Sunday, June 23d, a more complete eruption appeared on the face and neck, and she was relieved from the violence of the fever. The vomiting however continued, the throat became very sore, and a salivation began.

Monday, June 24. The eruption extended itself on the body, the fever was still more abated, but the salivation, soreness of the throat, and vomiting, were urgent symptoms.

Tuesday, June 25th, the fourth day of the eruption. The salivation and retching continued, with soreness of throat.

Wednesday, June 26th, fifth day of the eruption. Pustules were noticed on the lower extremities, those on the face advance, and the eyes are swelled; the number of the pustules on the head and face are about two dozen.

Thursday, June 27th, sixth day of the eruption. The pustules on the face begin to turn. She still suffers from sore throat and salivation. This evening, contrary to advice, she went to her business at the theatre.

Friday, June 28th, seventh day of the eruption. The pustules on the face are turned, those on the lower extremities are few in number, but well filled, and not yet changed.

Saturday, June 29th, eighth day of the eruption. She only complains of sickness. After this day the pustules turned and dried on the lower extremities, and no complaint remained.

This case appeared to have been a very mild case of distinct smallpox.

ROBERT BREE.

This case was visited by the greater number of members of the board, and also by the director, and was attended by Mr. Hewson, of James street, Covent Garden, who entertains no doubt of this having been a case of distinct smallpox.

III. *Case of John Godwin.*

Mrs. Godwin, No. 6, Stratton street, Piccadilly, states, that she was brought to bed of this son in October, 1800; that six weeks after he was born, the smallpox prevailed very much in her neighbourhood, and one child died of it in the house in which she lived. About this time her son was attacked with very violent fever, succeeded by a copious eruption all over the face and body, which was declared by Mr. Smith, an apothecary who attended him, to be the smallpox, and which was ten or twelve days before it completely scabbed and dried off.

Some time after this, a brother of her husband, a medical

man, who had not seen the child during its illness, inoculated him for the smallpox, in order to insure his complete security; a small pimple on the part was only formed, which soon disappeared, and no fever or eruption ensued. About six weeks ago, this boy, now eleven years old, was attacked with fever, followed with an eruption, which broke out on the face, body, and limbs, exhibiting the ordinary appearance of smallpox, and which turned on the eighth day.

Mr. Kerrison, of New Burlington street, who attended this boy, states, that the eruption exhibited the exact appearance, and passed through all the stages of distinct smallpox. He also from this boy inoculated a child who had fever at the usual time, followed by a slight variolous eruption.

The history of the former disease was procured from Mrs. Godwin, and the history of the second attack of smallpox from Mr. Kerrison, by Mr. Moore, director of vaccination at this establishment.

IV. *Case of Peter Sylvester, No.* 10, *Cross street, Carnaby Market.*

This boy's parents are both dead. He was born on June 7th, 1798, and on the 21st of February following was inoculated for the smallpox by Mr. Ring, of New street, surgeon. Mr. Ring showed the director of vaccination at this establishment, his account book of that period, in which there is a charge regularly entered for inoculating this boy for the smallpox.

The cicatrix on his arm is still conspicuous, and six or seven smallpox pits, occasioned by the former eruption, have marked his face.

On the 24th of June last, this boy was taken ill with fever; on the 27th an eruption on the skin took place. Mr. Moore, the director, saw him on the 30th: the spots on the skin were very numerous, but distinct, and the skin round their bases was inflamed; many had formed within the mouth and throat.

July 1st. The eruption has now assumed the appearance of genuine smallpox, the pustules are augmenting, and the face is beginning to swell. 2d. The pustules are larger, and the face much swelled. 3d. The pustules on the face are at the height,

and the eyes are nearly closed. 4th. The pustules on the face have all begun to turn; all fever is gone.

This case is drawn up from the notes of Mr. Moore. The case was visited by several members of the board, and by many other medical gentlemen of the highest respectability.

From the period at which the violent opposition to smallpox inoculation subsided, till the establishment of vaccination, no reasonable parent has refused to allow his children the benefit of inoculation, although it has been generally acknowledged that the inoculation of the smallpox sometimes produces a fatal disease; and if at that time the instances in which the natural smallpox had occurred after inoculation, had been communicated to the public, every intelligent man would undoubtedly have still continued the same course, from a desire of affording his children the best chance of safety, although his confidence in the absolute security from natural smallpox must have been in some degree abated.

In the same manner, no effect injurious to vaccination ought to result from the knowledge of the above failures. Parents had been always apprised that there were occasional failures of vaccination, but they were always aware that none of their children would die of vaccine inoculation; and that when it failed, the succeeding smallpox was almost always much mitigated and disarmed of half its terrors. It was natural therefore, that they should choose vaccination as the less dangerous disorder, and the same reason still exists for their perseverance in that choice. If there be constitutions, which are twice susceptible of smallpox, a disorder which produces a violent action upon the human frame, and often destroys life, it is natural to expect that vaccination should not in every instance prevent the smallpox, and that the anomaly which occurs in the one disease should likewise take place in the other. It is ever to be kept in view, that the number of deaths from inoculated smallpox, exceeds the number of failures of vaccination. It appears from the present state of our information, that one person in three hundred dies from the inoculated smallpox, and that there is perhaps one failure in a thousand after vaccination. An individual, who, under such circumstances, should prefer the inoculation of his children for the

smallpox, to submitting them to vaccination, would be guilty of an improvidence similar to that of a parent who should choose for his son a military service, in which there was one chance in three hundred of being killed, in preference to a station, where there was only one chance in a thousand of being slightly wounded.

The board are of opinion, that vaccination still rests upon the basis on which it was placed by the reports of the several colleges of physicians and surgeons of the united kingdom, which were laid before parliament in the year 1807. That the general advantages of vaccination are not discredited by the instances of failure which have recently occurred, the proportion of failures still remaining less in number than the deaths which take place from the inoculated smallpox. They are led by their information to believe, that since this practice has been fully established, no death has in any instance occurred from smallpox after vaccination. That in most of the cases in which vaccination has failed, the smallpox has been a disease remarkably mild, and of unusually short duration; and they are further of opinion, that the severity of the symptoms with which Mr. Grosvenor was affected, forms an exception to a general rule.

That absolute security from the natural smallpox is not even to be attained by smallpox inoculation, is sufficiently evident from the annexed cases; and the board are enabled to state, that they have been made acquainted with instances of individuals who have twice undergone the natural smallpox.

Under all these circumstances, the board feel justified in still recommending and promoting vaccination, and in declaring their unabated confidence in this practice. Since in some peculiar frames of constitution the repetition of smallpox is neither prevented by inoculation nor casual infection, the board are of opinion, that in such peculiar constitutions the occurrence of smallpox after vaccination may be reasonably expected, and perhaps in a greater proportion; but with this admission, they do not hesitate to maintain, that the proportionate advantages of vaccination to individuals and the public, are infinitely greater than those of smallpox inoculation.

They are anxious, that the existence of certain peculiarities

of the human frame, by which some individuals are rendered by nature more or less susceptible of eruptive fevers, and of the recurrence of such disorders, should be publicly known; for they feel confident that a due consideration of these circumstances, and a just feeling of the welfare of the community, will induce the public to prefer a mild disease like vaccination, which where it fails of superseding the smallpox, yet mitigates its violence, and prevents its fatal consequences, to one whose effects are frequently violent; to one which often occasions deformity and blindness; and, when it is contracted by casual infection, has been supposed to destroy one in six of all that it attacks. And it must not be forgotten, that in a public view this constitutes the great objection to inoculation of the smallpox, that by its contagion it disseminates death throughout the empire, whilst vaccination, whatever be the comparative security which it affords to individuals, occasions no subsequent disorder, and has never, by the most violent of its opposers, been charged with producing an epidemical sickness.

By order of the board,

JAMES HERVEY, Register.

July 18, 1811.

On the Progress and present State of the Practice of Vaccination.

By T. BATEMAN, M. D.

[From the Philosophical Magazine for October, 1811.]

THE objects which the general adoption of vaccine inoculation will accomplish for mankind, if time and experience shall confirm the promises of its benevolent discoverer, are so important, that every friend of humanity must have followed with anxious hope the progress of the practice, and rejoiced at the general result of the evidence in its favour. It is not easy, indeed, to calculate the sum of human misery that will cease to exist, when the prospect which vaccination holds out to us shall be realized. In its casual, or *natural* occurrence, as it is termed, the smallpox is not only the most loathsome distemper that visits the human frame, but the most fatal pestilence; sweeping off multitudes during its prevalence, and destroying

the sight, corrupting the habit, or otherwise inflicting disease on great numbers of those who escape its more destructive effects. The practice of inoculation had, it is true, already diminished those evils among the individuals who resorted to it; but it had unfortunately augmented the evils among the people in general, by the perpetual infection which it disseminated, and the artificial epidemic which it constantly kept up. In London, for instance, during the first thirty years of the eighteenth century, before inoculation could yet have had any effect, the proportionate number of deaths occasioned by smallpox, as stated in the bills of mortality, was about seventy-four out of every thousand: but during an equal number of years at the end of the century, the number amounted to nearly one tenth of the whole mortality, or ninety-five out of every thousand. So that, as far as we are able to judge from hence, the practice of inoculation, which in itself might be esteemed one of the greatest improvements ever introduced into the medical art, has actually multiplied the ravages of the disease which it was intended to ameliorate, in the proportion of above five to four.* And the extent of the mischief inflicted on the survivors is manifest from a statement published by the society for teaching the indigent blind, that nearly one fourth of the persons admitted into that charity have been deprived of their sight by the smallpox; not to mention the various forms of scrofula and other diseases which it frequently excites.

It is true, that the more intelligent classes of society, who have generally adopted the practice of inoculation, have in a considerable degree avoided the worst of these consequences of smallpox: they have seldom been deprived of the blessing of sight; and they have only been destroyed by the disease in the proportion of about one in three hundred. But the humane will shudder at the recollection, that this exemption has been obtained at the expense of so much additional misery inflicted on the people at large; and that they have but shifted a part of the evils from themselves, to be aggravated in the families of their less enlightened neighbours; while they perpetuate a plague, which would otherwise have had its periods of absolute cessation.

* See the Tables drawn up by Dr. Heberden, in his "Observations on the Increase and Decrease of different Diseases," &c. p. 36.

Such is the condition in which the most *improved* state of the art of medicine had placed us, before the benefits of vaccination were discovered; and such is the condition to which some persons would advise us to return, in consequence of the alleged insecurity of this preventive. But it would seem to be only necessary to take a clear and dispassionate view of the state of the facts, relative to the efficacy of the cowpox, up to the present time, in order to be convinced of its incalculable advantages, even were all the reported failures proved to have occurred; nay, if they had actually occurred to double the extent that has been represented. It is the purport of this paper to detail, in as brief a manner as possible, the sum of the facts which have recently been brought to light, and to point out the inference which seems to be justly deducible from them.

The national vaccine establishment, supported by parliament, has published two reports during the present year, containing the evidence which they have collected from various authentic sources. The colleges of physicians and surgeons at Edinburgh, and the faculty of Glasgow, have again given their decided testimony in favour of vaccination. They assert unanimously, that the practice of vaccination is generally approved of by the profession throughout Scotland; that no bad effects can be ascribed to the practice; and that, since its introduction into Scotland, the mortality occasioned by smallpox has very greatly decreased. The faculty of physicians and surgeons of Glasgow further state, that, since the middle of May, 1810, they have gratuitously vaccinated in their hall, 14,500 persons; and that, as far as is known, the "vaccination in all these has succeeded."*

The accounts from several public institutions, in and near London, are equally favourable.† In the royal military asylum

* Report from the vaccine establishment, 1811.

† 1811. It appears, that since the last annual report of the London vaccine institution, there have been inoculated by Dr. Walker, - - 2,490

From the commencement of the institution in 1806, - - -	8,595
By the appointed inoculators in the metropolis last year, - -	1,046
From the beginning, - - - - - -	3,109
By the appointed inoculators in the country, - - - -	20,801
From the beginning, - - - - - - - - -	177,474
Last year, charges of matter, - - -	31,992 to 6,539 applicants,
From the commencement of the institution,	93,080 to 18,900 applicants.

for the children of soldiers, where between eleven and twelve hundred are now received, vaccination has been practised since its first establishment in the year 1803. From that period to the present time, but *one* instance of death from smallpox has occurred; and it is worthy of remark, that the individual had not been vaccinated, in consequence of a declaration of the mother, that he had passed through the smallpox in his infancy. Vaccination was introduced into the foundling hospital in the year 1801; and every infant, soon after its admission, has since that period been vaccinated. From the commencement of this practice to the present time, no death has occurred from smallpox; and in no instance has the preventive power of vaccination been discredited, although many children, as a test of its efficacy, have been repeatedly inoculated with the matter of smallpox, and exposed to the influence of its contagion. A similar success has attended the practice of vaccination at the lying-in charity of Manchester, where, in the space of nine years, more than nine thousand persons have been effectually vaccinated, and secured from the smallpox. The officers of the vaccine establishment in London, through the medium of their correspondence with many similar establishments in the country, have learned, that practitioners of the highest respectability are earnestly engaged in promoting the extension of the practice; that, among the superior classes of the people, vaccination is every where generally adopted; and that, although the prejudices of the lower orders, which have been excited by interested persons, still exist, they appear to be gradually yielding to a conviction of its benefits. This inference is likewise confirmed by the fact, that 23,362 charges of vaccine matter have been distributed by the establishment to various applicants from all parts of the kingdom, which exceeds, by nearly one third, the number distributed in the preceding year.

Of the immense benefits resulting from the universal adoption of vaccination in other countries, the accounts from India have furnished the most interesting example. The number vaccinated in the island of Ceylon, from the year 1802 to January 1810, amounts to no less than 128,732 persons; and the smallpox has literally been exterminated from the island.

From the month of February, 1808, to the last mentioned date, the disease had not existed in any part of the island, except in October, 1809, when it was carried thither by a boat from the Malabar coast: but, in this instance, the contagion spread to only six individuals, who had not been vaccinated, and was immediately arrested in its progress, and disappeared. The medical superintendant-general observes, that they have no apprehension that the smallpox will ever spread epidemically in Ceylon, while vaccination continues to be generally practised; at the same time, that its occasional appearance there has the good effect of proving the preservative power of the vaccine pock, and of rousing the natives from their apathy on the subject. Even the Bramins are now surmounting the prejudices of their education, and submitting to be vaccinated.*

It appears from a report of the central committee of the vaccine institution at Paris, published on the tenth anniversary of its establishment, that the benefits of vaccination, in augmenting the population of a country, have not escaped the attention of the present ruler of France, who has formed depôts of vaccine fluid in twenty-four of the principal cities, communicating with the central committee at Paris. In some of the departments, it is said, the zeal of the prefects has been such, that there remain none to vaccinate but the infants born in every year, and that the smallpox is already unknown. And the returns of the mortality in the city of Paris, for the year 1809, exhibit only 213 deaths by smallpox, "This number," say the reporters, "though yet too considerable, since the vaccine offered to these 213 victims a certain method of preservation, is yet extremely small in comparison of that of some years, when the epidemic smallpox has carried off, in the same city, more than 20,000 individuals." The committee, consisting of sixteen of the principal physicians of Paris, express their conviction of the efficacy of vaccination in these terms: "Ten years of labour and success have at length decided the important question, as to the vaccine possessing the power of preserving all those, in whom it has regularly gone through its progress, from the smallpox. This has been carried to such

* See the Report from the vaccine establishment.

a degree of certainty by the experiments of the central committee and its numerous correspondents, as well Frenchmen as strangers, that there is not at present any fact in medicine better proved, or more certain, than that which establishes the truly *antivariolous* power of the vaccine."*

Such is the result of the progressive experience of professional men, in regard to the efficacy and preventive powers of vaccination: such is the confirmation, which the inferences, drawn from the early investigation of this subject, have received from subsequent and more extensive research! Insomuch, that the conclusion of the college of physicians upon the subject, in the year 1807, must now be deemed indisputable, that "the truth seems to be established as firmly as the nature of such a question admits."†

The opposition to the practice, which is still but too successfully kept up by a few clamorous individuals in the medical profession, rests principally upon a mistaken view of the nature of the question. It rests upon the notion that the result of the practice should be uniform and invariable; that the *rule* should be void of all *exceptions*. But there is no such regularity in the operations of the animal economy: there is no disease without its anomalies; and the diversity of human constitutions is infinite. Several of these anomalies, or exceptions to the general rule, have doubtless occurred in the practice of vaccination; "but," to use the words of a judicious and experienced observer, "certainly not so often as was expected by those who considered the subject from the first dispassionately, nor have they been in sufficient number to form any serious objection to the practice founded on Dr. Jenner's discovery."‡ In truth, if this principle were received,—that no operation ought to be performed on the human body which was liable to occasional failure,—what medicine would remain for us to exhibit, or what surgical assistance for us to offer?

But let us examine the nature of these exceptions, or "fai-

* A copy of this report may be found in the Edinburgh Medical and Surgical Journal, for January, 1811. p. 117.

† See the report of the royal college of physicians on vaccination, July, 1807.

‡ See Dr. Willan's Treatise on Vaccination.

lures" as they have been emphatically called, which have occurred in the practice of vaccination. The very sound of the word excites an alarm in the minds of many persons, as if *failure* were synonymous with *death*, or implied the certain occurrence of a desperate or mortal smallpox. But this is so far from being the case, that upon a deliberate view of the facts, we do not hesitate to affirm, that, if all the cases of alleged failure, which the opponents of vaccination have raked up, upon any sort of evidence, and often upon none, had really occurred, and that number had been doubled or tripled, its advantages over the inoculation of smallpox would still be incalculable.

In the first place, it has been ascertained by the concurring observations of almost all the practitioners who have attended to the subject, that (to use the words of the college of physicians) "in almost every case in which the smallpox has succeeded vaccination, whether by inoculation or by casual infection, the disease has varied much from its ordinary course; it has neither been the same in *violence*, nor in the *duration* of its symptoms; but has, with very few exceptions, been *remarkably mild, as if the smallpox had been deprived by the previous vaccine disease of its usual malignity*."* Dr. Willan states, that the feverishness which precedes the eruption in these cases is often considerable, but the pustules are small and hard, containing little or no matter, and begin to dry off on the sixth day.† It must not be omitted, indeed, that in a very few instances the smallpox subsequent to vaccination has assumed the confluent form, and put on a dangerous aspect (as in the recent case of the son of earl Grosvenor); but even in these rare instances, the modifying influence of the previous vaccination has been manifest, the disease, when near its height, receiving a sudden check, and the recovery being unusually rapid.‡ One case of this sort occurred to the observation of the writer of this paper, in which, on the seventh day of confluent smallpox, the child became suddenly free from constitutional complaint, and ran about at play; a circum-

* See the report of the college. † See his Treatise, sect. iv.

‡ See the last report of the national vaccine establishment, July, 1811.

stance, he believes, that is never known to occur in confluent smallpox where the previous influence of vaccination had not been exerted. In this statement, then, we have admitted the worst consequences that have ever accompanied the "failures" of vaccination, in any one instance.

But, in the second place, let us attend to the proportionate number of these failures. "It does not appear," says Dr. Willan, who minuted the cases as they happened, "that failures in the preventive effect of vaccine inoculation, including *mistakes*, *negligences*, and *misstatements*, have occurred in a greater proportion than as *one* to *eight hundred*."* It is very improbable, then, that the actual failures amount to one in a thousand, or to any thing near that number. But let us suppose, for the sake of argument, that the failures amount to the proportion of one in five hundred; that is to say, that one of every five hundred persons vaccinated remains liable to be infected by smallpox: and let us further imagine, that this subsequent smallpox is not mitigated in any case, and therefore, that (as in the case of the ordinary *natural* smallpox) *one* in *six* of these will die. Then the worst result would be, that *one* out of every *three thousand* persons vaccinated would die. But we know, that *one* of *three hundred* persons, who receive the smallpox by inoculation, perishes of that disease.† The conclusion is therefore obvious, that the worst result that could be calculated upon from vaccine failures, would leave the balance in favour of vaccination, in the proportion of *ten* to *one*. But when we consider the actual state of the circumstances; that the number of deaths from inoculated smallpox really exceeds the number of "failures" of vaccination; that these "failures" are, in a great majority of instances, the means of insuring a very mitigated and harmless smallpox; and that they have, perhaps, in no instance, been followed by a fatal smallpox; the chances of fatality from a failure of the vaccination are so trivial as to elude calculation; and the only chance of injury that ensues, is reduced to that of a temporary inconvenience.

* See his Treatise, p. 23.

† Dr. Willan states, that "the inoculated smallpox still preves fatal in *one* case out of *two hundred and fifty*.—Ibid.

Lastly, let us reflect on the noncontagious nature of the vaccine disease, which, while it secures the individual from blindness, deformity, or fatuity, too often consequent on the smallpox, injures no one, and spreads no epidemic around, and we shall be compelled to admit, that, "with all its imperfections on its head," with a frequency of failure that its opponents have never yet ascribed to it, vaccination would still prove a blessing, such as few individuals have had the happiness to confer upon mankind.

We might here have terminated our observations, but the leading circumstance, communicated in the late report from the national vaccine establishment, demands some notice. It is singular, that at the time when the public attention was attracted by the occurrence of smallpox after vaccination, in the sons of the earl of Grosvenor and sir Henry Martin, the second occurrence of smallpox in the reverend Joshua Rowley, miss Booth, and two other persons, should have happened. In three of these cases, the previous smallpox had been taken by inoculation, and in the fourth, in the natural way. But the truth is, that the smallpox itself, in whichsoever of these two ways it is produced, is liable to the same anomalies and exceptions as the cowpock. There are several examples of the fact on record; one of the most striking of which is the case of Mr. Langford, related in the 4th volume of the Memoirs of the Medical Society of London. This person was so "remarkably pitted and seamed" by a former malignant smallpox, "as to attract the notice of all who saw him:" yet he died at the age of fifty, in an attack of confluent smallpox, in which he communicated the infection to five other individuals of the family, one of whom also died. It will be unnecessary here to detail the various examples which authors have described. The writer will just notice an instance which occurred under his own observation not long ago, the particulars of which will be detailed in the second volume of the "Medico-Chirurgical Transactions" about to be published.* This occurred in a woman of twenty-five years of age, who was considerably pitted by a former confluent smallpox, which she had suffered

* Several cases and many references will be there found, which are omitted here for the sake of brevity.

in her childhood. She caught the second disease, which went through the usual variolous stages in a mild way, by nursing her infant under a confluent smallpox, which proved fatal to it. It is remarkable, that her two elder children, who had been vaccinated a few years before, lived in the same apartment, during the progress of the smallpox in the infant and mother, and escaped the infection; the cowpock in them having exerted a preventive power, which the previous smallpox had failed to effect in the mother. The poor woman had been prevented, by the terrors excited by the anti-vaccinists, from vaccinating her youngest child: a fact which should induce these opponents of the practice to reflect on the serious responsibility which they assume, in thus discouraging the adoption of this important preventive. I am, &c.

T. BATEMAN, M. D.

Bedford Row, August 19, 1811.

Some Remarks on the Physiology of the Egg: communicated in a Letter from John Ayrton Paris, *M. B. to* William George Maton, *M. D. V. P. L. S. &c. &c.*

[From the Philosophical Magazine for May, 1811.]

DEAR SIR,

THE extensive range which the *ovipari* form in the scale of animated existence renders the physiology of the egg a subject of extraordinary interest and importance to the disciple of Linnæus: I am therefore induced to hope that the communication of any new facts relative to its organization and development will be received by you as an acceptable tribute to the cause of natural history.

The *ova*, or germs of oviparous animals, admit of an evident division into two orders. I. The Perfect, and II. the Imperfect. The former are deposited by the *aves*, *serpentes*, and by most *oviparous quadrupeds*, and are completely formed *in utero;* whilst the latter, produced by some of the *testacea*, *amphibia*, and by most *pisces*, acquire additions after their exclusion. The observations contained in this memoir relate more particularly to the class *aves*, the history of whose *ova*

comprehends whatever is interesting or important in the germs of inferior animals. The egg, when completed and deposited, consists of the following parts:

1. *Vitellus* or *yolk*, with its *capsule* and *cicatricula;* 2. The two *albumina*, with their proper membranes; 3. The *chalazæ;* 4. The *folliculus aëris;* 5. The *common membranes;* 6. The *exterior involucrum*, or *shell.*

The necessity of any description of these parts is superseded by the minute and valuable details which are to be found in the works of Fabricus ab Aquapendente, Harvey, Malpighi, and of many modern and enlightened physiologists; I shall confine myself, therefore, to what I consider exclusively original.

The principal use of the albuminous portion of the egg is doubtless to afford materials for the growth, and nourishment for the support, of the ovular embryon: such however does not appear to be the *only* purpose for which it is designed. No where does nature display more anxiety for the preservation of her offspring, or more wisdom to obtain her objects, than in her provisions to ensure an equitable temperature to the *fœtus in ovo:* a condition which is so essential to the evolution of the animal, that the smallest deviation overthrows the nice balance between the different actions that are to mature it, and produces fatal effects. The *albumen* then I consider as a great defence against such an evil. The *chalaza*, by retaining the *cicatricula* at the source of heat, obviates the mischief that would accrue from constant change of position; but the *albumen*, being a most feeble conductor of caloric, retards the escape of heat, prevents any sudden transition of temperature, and thus averts the fatal chills which the occasional migrations of the parent might induce. As an illustration of the use and importance of such a structure, I may observe, that those fish which retain their vitality a considerable time after their removal from the water, as eels and tench, have the power of secreting a slimy and viscid fluid, with which they envelop their bodies. Is it not extremely probable that this matter, by acting like the albumen of the egg, and preventing evaporation from the surface of the animal, and the consequent change of temperature, may be the principal cause of this tenacity of life?

It must however be remarked, that deviations of temperature are injurious and fatal in proportion only to the degree of vital energy which the ovular embryon possesses: hence germs of inferior vitality not only suffer the vicissitudes of heat and cold with impunity, but are developed by a less defined temperature. We therefore perceive, as we descend the scale of oviparous beings, that those peculiar provisions which the eggs of perfect animals possess, for the regulation of their temperature, cease to be essential, and therefore disappear.

The part of the egg to which I next beg to direct your attention is the *folliculus aëris*, or air bag, placed at its obtuse extremity; the nature of this follicle excited in me considerable interest, as I found that it had not been so fully investigated as its importance seemed to demand.

The external shell, and the internal membrane by which it is lined, constitute the *parietes* of the cavity, whose extent in the recent egg scarcely exceeds in size the eye of a small bird: by incubation, however, it is extended to a considerable magnitude. That its most essential use is to oxygenate the blood of the chick, in my opinion there can be no doubt: but to establish completely the truth of such a theory, it is necessary to discover the nature of the air by which it is inflated, and which has hitherto remained unexamined. We are informed by Buffon, that it is a product of the fermentation which the different parts of the egg undergo. If the count's conjecture be established, it must be nonrespirable, and therefore cannot discharge the office which such a theory would assign to it. To determine this matter, and to discover also whether the process of incubation produces any change in its chemical constitution, I instituted the following experiments, viz.

Experiment I. Twenty-one hen's eggs newly laid, when punctured at their obtuse extremity, yielded only one cubical inch of gas, which, when received in a jar, and subjected to the eudiometric test of Dr. Priestley, I found to be pure atmospherical air.

Experiment II. Two eggs, after twenty days' incubation, were opened under the surface of water, from which one cubical inch of gas was collected: this I also discovered to be at-

mospherical air, contaminated however with a small portion of carbonic acid, which I suspect to be derived from the venous blood of the chick, and which seems to establish another most beautiful analogy between this mode of oxygenation, and respiration after birth.

From these results the following corollaries may be drawn, viz.

1. The *folliculus aëris* before incubation contains atmospherical air.

2. No other chemical change takes place in the constitution of the air, than a small inquination with carbonic acid.

3. It gains by incubation an increase of volume, which takes place nearly in the ratio of ten to one.

I must here remark, that its extent does not increase equally in equal successive portions of time, but observes a rate of progression, which is accelerated as the latter stages of incubation advance: it seems, however, to arrive at its *maximum* of dilatation a few days previous to the exclusion of the animal.

In the eggs of inferior animals, the embryon does not appear to be oxygenated by any distinct apparatus, but, like the animal which it is hereafter to become, receives air through the medium of *spiracula*, dispersed over the exterior *involucrum*. The description of the *folliculus aëris* just delivered is taken from that in the egg of our common hen. The same apparatus exists in the eggs of all birds, and contains a similar air: its capacity, however, does not seem to vary either with the size of the egg, or of the bird to which it belongs; but I think I have discovered a beautiful law by which its extent is modified.

I have uniformly found, as far as my contracted inquiries have led me, that the *folliculus aëris* is of greater magnitude in the eggs of those birds which place their nests on the ground, and whose young are hatched fledged, and capable of exerting their muscles as soon as they burst from their shell, than in the eggs of those whose nests are generally built on trees, and whose progeny are born blind and forlorn. Thus the *folliculi* in the eggs of fowls, partridges, and moor-hens are of considerable extent, whilst those in the eggs of crows, sparrows, and doves are extremely contracted. The chick, therefore, of

fowls and patridges has a more perfect plumage, and a greater aptitude to locomotion, than the callow nestlings of doves and sparrows. Such an instance of the agency of oxygenation in the promotion and increase of muscular power is not solitary in physiology; for the history of ruminating animals will furnish us with a parallel example. "Their cotyledons," observes the author of Zoonomia, "seem to be designed for the purpose of expanding a greater surface for the termination of the *placental* vessels, in order to receive oxygenation from the *uterine* ones: thus the progeny of this class of animals are more completely formed before their nativity than that of the carnivorous classes. Calves therefore and lambs can walk about in a few minutes after their birth; while kittens and puppies remain many days without opening their eyes." If any further testimony be necessary to show that the augmentation of muscular energy is the result of a nice combination of oxygen with the animal organs, many interesting facts might be adduced in confirmation of its truth. We generally find the strength of an animal proportionate to the extent of its chest: hence an attention to the "*animosum pectus*" has been attended with the improvement of our breed of cattle; and it is in consequence of a great extent of pneumatic receptacle that birds are enabled to bear the prodigious muscular exertion of flight. Is it not probable too, that the repeated suspirations of the fatigued are instinctive exertions to procure a greater proportion of oxygen, by which their muscular energy may be revived? I must not quit the subject of this follicle, without noticing a very curious fact well known to every one employed in the concerns of a farm-yard, that, if the obtuse extremity of an egg be perforated with the point of the smallest needle, (a stratagem which malice not unfrequently suggests), its generating process is arrested, and it perishes like the *subventaneous* egg. Hence sir Busick Harwood was led to suspect that the elastic fluid contained in the air-bag was oxygen, and I was induced to examine its nature. Can this curious problem be solved, by supposing that the constant ingress of fresh air is too highly exciting? A parallel example may be adduced from the vegetable kingdom in support of such an opinion. The young and tender plant, before it puts forth its roots, is often

destroyed by having too free a communication with the atmosphere, by which its powers are exhausted: it is to obviate such an effect, that the horticulturist, taught only by experience, covers it with a glass, by which he limits the extent of its atmosphere, and consequently decreases its respiration, transpiration, and the inordinate actions which would prove fatal to it.

I shall close this paper with a few observations on the formation of the exterior *involucrum*, or shell, by which this microcosm is defended from external violence. We here detect a single operation, at once answering two of the wisest and most important purposes of the animal: it at once averts destruction from the individual, and contributes essentially to the preservation of its species; for, whilst it removes the calcareous matter, which, if allowed to accumulate, must render the bird incapable of flight, and defeat the best purposes of its existence, it furnishes the germ of the future animal with a strong and convenient defence. The eggs of birds are, however, sometimes destitute of this provision, which I think may arise from the secretion of calcareous matter not keeping pace with the exuberant production of the fluids of the egg. Hence we perceive this imperfection oftener occurring in strong birds, and in the months of harvest, when their food is more luxuriant and abundant. The experiments of Vauquelin, which prove that the quantity of calcareous matter voided by birds exceeds that taken in, suggested to Fordyce, that birds must require calcareous matter during their laying, and that, if the animal be deprived of it, the shell is never formed. Such a theory, however, is not only derogatory to the wisdom of nature, but illegally deduced from the experiments themselves. Are we to expect, from our imperfect notions of elementary bodies, to explain the origin of every substance found in the animal economy, or the series of changes which it undergoes! Nature has her own laboratory, and is capable, without any foreign aid, of preparing the ingredients necessary for her productions. That a deficiency of calcareous matter in the system is the cause of the absence of the shell, no one will deny; but that this depends on some internal state, and not on the privation of lime, may be shown by the following curious circum-

stance. A hen, which I kept for some experiments, had its leg broken in two parts. The fracture was carefully bandaged; three days subsequent to which, several eggs destitute of shells were found on the premises. The hen had deposited no perfect eggs, nor were there any other birds from which these yolks could have proceeded: I therefore conjectured that all the *calcareous matter* designed for the formation of the shell had been employed in the regeneration of the bone. We find a similar law existing in the human species. The reunion of a bone fractured during a woman's pregnancy is often delayed until her delivery; and it is well known, that, if the horns of a deer be broken at the rutting season, it is incapable of procreating its species.

I remain, dear sir, with great esteem,

Yours, faithfully,

JOHN AYRTON PARIS.

Westminster.

A concise Description of Schooley's Mountain, in New Jersey, with some Experiments on the Water of its Chalybeate Spring. By Samuel L. Mitchell, *Professor of Natural History in the University of New York, Representative in the Congress of the United States, &c. &c. Communicated by the Author.*

[From the Philosophical Magazine for March, 1811.]

THERE had been so much conversation about Schooley's mountain, that in the beginning of July 1810, I executed the desire I had long entertained of visiting it.

Schooley's mountain is a part of a chain which extends in a north-easterly and south-westerly direction across the state of New Jersey. It may be traced from the Highlands of New York. Towards the Hudson, its ridges divide the plains of Rockland county from those of Orange, being denominated the Haverstraw, Warwick, Skunemunk, and Stirling mountains, and being distinguished locally by several other names. Towards the Delaware, it separates the upper waters of the Raritan from those of the Musconetcunck, and passing from Sussex through Morris and Hunterdon counties, is called,

somewhat to the southward of Philipsburg, the Musconetcunck mountain. The more noted portion of its middle region is termed Schuyl's Hills, or Schooley's Mountain. The latter name is the most prevalent, and is derived from a family which was formerly a considerable proprietor of the soil thereabout. The former appellation is probably a mere abbreviation or corruption of it.

This ridge discharges the water from its north-west side, partly through the Walkill, into the Hudson, a little to the eastward of Esopus, after traversing Sussex county, in New Jersey, and Orange and Ulster, in New York. Part also empties into the Hudson through Murderer's Creek, at New Windsor. Another portion is collected into the Musconetcunck river; and running almost parallel with the mountain, falls into the Delaware, not many miles south of Eastown. The water from the south-east side feeds the upper streams of the Passaick, which, after visiting Orange, Rockland, Morris, Essex, and Bergen counties, falls into Staten Island sound, to the southward of Newark. The stream called the Black river beyond Mendham, and that termed South-branch, watering Dutch valley, neither of them reach the Delaware, but empty into the Raritan, some distance above Brunswick.

Thus these heights completely divide the waters of New Jersey. Not a single stream is known to pierce them. From their north-western slope, all their streams find their way into the Hudson and Delaware. From their south-eastern declivity, their currents travel to the ocean by Newark and Raritan bays. They have, however, no pretensions to be classed with the Shawangunk mountains, which are a distinct chain, and make part of the great Allegany, that traverses the continent to the confines of Georgia. Nor have they any connexion with the Kaatskill mountains, which are themselves quite detached from the Shawangunk. Schooley's mountain is of more moderate elevation than either. Geometrical measurement has ascertained that the height of Schooley's mountain above its immediate base is more than six hundred feet. And a calculation made by approximation, on the falls of water at the different milldams along the hurrying channel of the Musconetcunck, to its junction with the Delaware, and on the descent

thence to Trenton, makes the position of that base to be nearly five hundred feet more above tide-water. The elevation above the level of the ocean does not, therefore, in all probability, much exceed eleven hundred feet. And this is about the height ascribed to Anthony's Nose, in the Highlands of New York, by Mr. Knight.

The elevation is, nevertheless, considerable enough to influence its temperature. The heats of summer are not so great as in the valleys. Droughts are less common and pinching. Snow falls earlier, and lies longer than in the adjacent plains. The warmth of a copious spring of pure water, as it issued out of the sand near the top of the mountain, was only 50 degrees, while the temperature of the water gushing from the briskest springs on the north side of Long-island, and drawn from the deepest wells at New-York, is 54 degrees. The spring water on the summit of Schooley's mountain is, therefore, four degrees colder than that around New York.

This mountain is not a mass of stratified rocks, piled upon each other from bottom to top. There is no peculiar difficulty in travelling over it. The predominating materials are clay and sand, forming a good loam; which, though generally not argillaceous enough for the formation of bricks, is, at the same time, gravelly enough for the growth of grass and grain. Yet rocks are thickly distributed over its face and along its sides. They are mostly detached, though some of them are of large dimensions. They consist chiefly of feldspar and quartz: the quartz is prone to be semipellucid, and is granular or angular, resembling coarse marine salt. The feldspar is mostly whitish, sometimes reddish, and presents less of the polished fracture than the American feldspars usually do. It has the appearance of a more imperfect formation, or of having undergone a partial decomposition. These two ingredients make up the bulk of the rocks. Many masses may be examined without observing a vestige of mica. Abundant as mica is almost every where in these parts, with the mixtures of feldspar and quartz, in our primitive rocks, it is remarkably deficient here. Now and then a little schistus, or horneblende, is found embodied and compacted with the quartz and feldspar. Grains of yellow pyrites also somtimes occur. Rust, ochre, and other indi-

cations of iron, are dispersed extensively both through the rocks and the soil. Iron ore is indeed so plentiful, that furnaces are in operation both in the eastern and western districts of the chain. Much of it is magnetical, and its action is so powerful upon the needle, that surveyors of land often find it very difficult to employ the compass. It would be possible to collect great quantities of the magnet, and of other ores of iron in the middle region. Towards the foot of the hills, limestone is found skirting the valleys along, and is calcined in quantity sufficient for all economical uses.

Among the natural productions thereabout, are masses of excellent flint stones. They lie along the valleys and side hills, where they have been washed bare; and are sufficient in quantity and quality for domestic supply of our musketry. They are more pure and of a better fracture than those contained in the limestone near Niagara. And when this important article of public defence shall be thought worthy of being improved by the citizens, there seems to be in New Jersey an inexhaustible supply for our fire-arms.

A turnpike road has been completed from the city of Jersey, at Powleshook, to the summit of Schooley's mountain. The travelling is excellent the whole distance. This is just fifty miles from New York city. Estimating the width of the Hudson to be two miles, the distance to Newark is nine, to Springfield seven, to Chatham five, to Morristown seven, to Mendham six, to Blackriver six, to Dutch-valley five, and to the Mineral Spring on the eastern or further side of the mountain, three miles. Through such a succession of thriving villages, and amidst a country pleasingly checkered with forests and farms, the rise of the first five hundred feet is surmounted in about forty-seven miles, as the traveller passes over a surface of easy elevations and depressions. The remaining six hundred feet are ascended in less than two of the remaining miles, between Dutch-valley and the summit. The principal part of the remainder is a descent to the spring on the opposite declivity.

An able horse will carry a chair hither from New York in a summer's day, or return thence between the rising and setting of the sun. From the top of the mountain one finished turnpike is continued northward, to Sussex, another westward, to

Eastown, and a third eastward, to New York. It is in contemplation to open a fourth from the same point, to proceed in a course southwardly direct to Trenton.

The Mineral Spring which has been mentioned has given much celebrity to the neighbouring region. It is said to have been known to the native Indians, and to have been employed by them as a remedy. The white people have resorted to it almost ever since the settlement of the country. Remarkable cures are ascribed to it: and some persons have been in the habit of visiting it season after season, for the purpose of being benefited by its wholesome properties.

It is situated in the town of Washington, in the county of Morris. It is, in strictness, a rill which issues from a fissure in the perpendicular side of one of the above described rocks, on its eastern exposure. The place of discharge is, perhaps, between forty and fifty feet above the level of a brook which gurgles over the stones, and foams adown the rocks in its channel beneath. The extremity of a wooden leader is so adapted to the crack in the rock as to receive the water, and convey it to the platform where the drinkers assemble, and to the recesses whither the bathers retire.

Its temperature is rather more than six degrees warmer than that of the spring water near the summit. The mineral water, as it pours from the spout, possesses a heat somewhat warmer than 56 degrees. This is about the same which the slower springs and the shallower wells around New York possess.

The quantity of water which it affords can easily be measured. By experiment, it appeared to discharge a gallon in about two minutes and a half. At this rate, the amount would be twenty-four gallons per hour. But allowance is to be made for leakage and waste, inasmuch as the conduit does not collect the whole. Suppose this to be six gallons more. Then the quantity running out will amount to thirty gallons per hour. Some trials are reported to have shown a rather more abundant flow. On the whole, it may be stated with tolerable correctness that the fountain within the bowels of the mountain emits, from this opening, a quantity of water not varying greatly from a barrel per hour, or six hogsheads per day. The

quantity is not observed to vary under any changes of season or weather.

The spouts which convey the water are lined with a yellowish deposit. The like sediment incrusts the reservoirs at the bathinghouse. The earth and stones through which the water soaks away, present a similar ochreous appearance. Where the boards contain astringent matter, a dark purple or blackish colour is formed.

The presence of iron being thus indicated, a few experiments were made to determine the matter more clearly.

A bright blue was produced on adding the prussiate of potash to the w ter.

Green leaves of the common chesnut tree, (*fagus castanea,*) on being bruised and infused in the water, formed a pale purple.

Those of chesnut oak (*quercus prinus monticola*) yielded a brighter purple.

Those of the sumach(*rhus glabrum*)quickly turned to a purple.

Fresh lacerated leaves of the maple (*acer rubrum*) immediately formed a deep purple.

Hickory leaves (*juglans vulgaris*) made a faint dusky hue.

Black oak leaves (*quercus nigra*) struck a darker colour.

Butternut leaves (*juglans cinerea*) afforded a dusky brown.

The waters of the spring, mixed with brandy, made a mixture of a dark and unsightly colour.

An infusion of green tea formed browns, purples, and blacks, according to its strength and proportion.

The chalybeate character of the water being thus established by so many tests, attempts were made to ascertain whether there was any gaseous impregnation.

For this purpose glasses were inverted in a convenient vessel, receiving the stream immediately from the spout. But not a bubble of air was collected, other than common spring water affords.

To determine whether any carbonic acid was combined with the water in a form not spontaneously separable, lime-water was mixed with it; but no change of colour was perceptible in the mixture.

Various proportions of the spring-water and lime-water were mingled in repeated experiments, without effecting any cloudiness or causing any precipitate.

Afterwards, as a test to the goodness of the lime-water, the milky hue and carbonic precipitate of the lime was instantly produced, by breathing through a tube into the mixture of waters, air which had undergone the respiratory operation of the lungs.

There was thus no evidence of any carbonic acid at all.

When the water of the spring was suffered to stand in the open atmosphere, and acquire the summer temperature, by receiving twenty or more degrees of heat, some air bubbles were distinguishable on the sides of the vessel; but they were only such as any cold water would exhibit under equal circumstances.

As there was no calcareous incrustation at the spring, there was reason to believe the water destitute of lime. On adding to it oxalic acid, there was no change of colour produced. Whence it may be inferred that lime makes no part of the constitution of this fluid.

To enable a judgment to be formed whether any other earths were combined with the water, soda and potash were severally and repeatedly added. The precipitates were, however, so small, and so slowly produced, that there was ground to suppose the presence of earthy matter was very inconsiderable, and that there was no metallic impregnation except that of iron.

The nitrate of silver caused a whitish appearance; but not in so considerable a degree as it does in the water of New York, constantly drunk by the inhabitants. The cloudiness was indeed not more considerable than rain-water along the sea-coast is occasionally known to present when subjected to the same test. The tinge of muriatic acid hereby indicated, probably arises from a faint solution of sea salt.

There is notwithstanding a weak acid of some kind in the Schooley's mountain mineral water. If litmus paper be exposed to the water as it issues from the rock, the blue is gradually changed to a reddish; and on the addition of an alkali, the acquired colour vanishes. What the nature of this inconsiderable portion of uncombined acid may be, is not perfectly easy to determine. Its presence is attended with the flavour which water derives from running over decayed leaves, and draining through a soil abounding with the living and dead

roots of trees, shrubs, and sylvatic plants. Former experiments have proved to me the existence of an acid in the rotten wood which overspreads the American forests. And water passing through a stratum of vegetable mould is known to receive what is called the *woody* taste. It is therefore presumable that the rain water receives a tincture from the thick layer of vegetable mould through which it is strained, and carries the flavour of it to the fountain. The peculiarities of this feeble acid, like that of numerous others we meet with in practice, does not seem to be defined in chemistry as yet by discriminating characters.

The iron of this mineral water is very easily separated. Exposure to the atmosphere is followed by a metallic precipitation. Transportation to a distance, as bottles are commonly corked, is attended with a deposition of the iron. The water, after having been carried to New York, when subjected to experiment in my house, gave no evidence of a chalybeate quality when tested by the Prussian alkali and spirituous tincture of galls. This same water, after being boiled in a kettle, makes excellent tea. The heat of ebullition seems to separate the ferruginous ingredient, and the infusion is thereby freed from all dusky or black tint. Still, if this same infusion of green tea is mixed with water fresh from the spring, a dark and disagreeable hue is instantly produced. A short exposure to the heat of 212° thus converts this mineral water into a good tea-water. It is employed for this purpose occasionally.

If there is any thing that deserves the name of a pure chalybeate water in the world, this would seem to be such a composition. The iron appears to be united with the water without the aid of carbonic, or indeed any other acid; for the weak acidity detected by litmus can scarcely be considered as contributing to its solution. Some part of the iron ore universally diffused among the minerals hereabout, is in a state proper for water to act upon, and to produce the martial impregnation remarkably free from other admixtures.

Schooley's Mountain, July 10, 1810.

Case of Poison by Arsenic. Communicated by Mr. Soden, *Surgeon, Coventry.*

[From the London Medical Review for April, 1811.]

MARCH 15, 1810, a little before eight o'clock in the evening, I was called to O. Y. aged 22. His father had discovered that he had purchased a quantity of arsenic since seven o'clock, and strongly suspected that he had taken some of it.

I found him vomiting, and complaining of dreadful pain in the stomach. His pulse was very rapid, and his legs were slightly convulsed. After some hesitation he acknowledged that he poured about half a pint of water upon a shilling's worth of arsenic; had stirred up the mixture, and swallowed it. An emetic, consisting of a scruple of sulphate of zinc, and two grains of tartarized antimony, was immediately exhibited. He discharged a considerable quantity of fluid from his stomach. The sickness was encouraged by repeated draughts of warm water, and milk and water. He suffered severely from a burning sensation in his stomach, and the vomiting continued till about half past nine; at which time he was attacked with diarrhœa, and constant inclination to void urine. The pain became intolerable in his bowels, the convulsive motion in his limbs were more frequent, and his pulse more feeble, but still very quick.

As it was obvious that part of the poison had reached the intestines, fifteen grains of scammony were given, a drachm of sulphuret of potash was dissolved in a quart of water, and he was desired to drink a cupful of this solution very frequently. A clyster of warm milk and water was thrown up to shield the coats of the intestines from the corrosive effects of the mineral, and his attendants were directed to inject some warm gruel into the rectum after each stool.

These means were diligently employed, but the dangerous symptoms increased with uncommon rapidity. The violent pain soon subdued him, his pulse sunk, his extremities were cold, and, about twenty minutes past eleven, after a dreadful convulsive laughter, his limbs became suddenly rigid, and he expired.

The body was opened about twelve hours after death, in the presence of Dr. Terry, Mr. Wilmer, and Mr. Tookey, of this city. The stomach and intestines appeared highly inflamed. The stomach was distended with a dark brown fluid: about two ounces of arsenic were found at its most inferior part. The vessels of the inner coat of the stomach looked as though they had been beautifully injected. The stomach generally was corrugated and puckered, but more particularly at that part which was in contact with the arsenic. The urinary bladder was contracted to the smallness of a walnut. The pleura of the lungs was with difficulty separated from the pleura costalis. The heart was firmly and universally united to the pericardium, and the pericardium to the pleura of the lungs. The brain was not examined.

Upon inquiry, it was ascertained that this unfortunate young man had purchased seven ounces of very finely powdered arsenic. It is impossible to state the precise quantity swallowed, as the paper containing a portion of the mineral was found under his bed, but was thrown away before I was informed of the discovery. There is however, reason to believe that he took more than four ounces of arsenic; a circumstance that readily explains the rapid termination of the case, and the highly inflammatory appearance of the viscera.

The subject, whose case I have narrated, passed my examination as a recruit for the army, and appeared healthy the day on which he died.

An Account of the Symptoms and successful Treatment of a Patient who swallowed a large quantity of Laudanum. Communicated by Mr. S. H. Murley, *Surgeon.*

[From the London Medical Review for October, 1811.]

A YOUNG woman, twenty years of age, on the 15th of July, 1811, took ten drachms of laudanum about eleven o'clock in the forenoon, which was not discovered till near two in the afternoon of the same day. The whole of the laudanum had been retained on her stomach, notwithstanding she had taken a strong solution of the sulphate of zinc. When I first saw her, about three o'clock, she appeared much convulsed, her hands

and jaws were firmly closed, her countenance of a ghastly paleness, and she was totally insensible. As the sulphate of zinc had produced no effect, a solution of the sulphate of copper with some ipecacuanha was given every ten minutes, until vomiting was produced. The temples and nostrils were constantly irritated with hartshorn, and after she had taken about three doses of the solution, vomiting came on, which in some degree roused her. By the assistance of two persons she was kept constantly walking or rather dragged about the room, and the operation of the emetic was promoted by draughts of warm water. Not the slightest smell or appearance of laudanum could be perceived in the fluid that was ejected. She complained of insupportable drowsiness, and weakness in her knees, urging her inability to walk, and requesting that she might be allowed to sleep. The emetic was repeated, but no laudanum was rejected. When the action of the emetic had ceased, I caused her to drink a considerable quantity of vinegar and water, still keeping her walking or in a state of agitation. At length she became less drowsy and towards evening could walk with little assistance, but appeared very much exhausted. A mixture with ammonia was now given, and she was permitted to lie down in bed, but her attendants were desired to rouse her frequently during the night. On the following morning she complained of much pain and confusion in her head, with numbness of her extremities, and her bowels were obstinately costive. A large blister was applied between the shoulders, and strong doses of purgatives were given, but without producing any discharge from the bowels. In the evening I ordered the abdomen to be well rubbed with salt and to be fomented, which speedily produced a copious discharge of dark-coloured and very offensive fæces. The numbness of the lower extremities, which indeed almost surmounted to paralysis, and an inability to contain her urine continuing, sinapisms were applied to her feet, which seemed to be of service. She remained for some days in a very lethargic state, with pain in the head, dimness of sight, and a constipated state of her bowels. These were relieved by the use of purgatives, and she is now perfectly well.

SELECTED REVIEWS.

A Treatise on the Process employed by Nature in Suppressing the Hemorrhage from divided and punctured Arteries; and on the Use of the Ligature; concluding with Observations on Secondary Hemorrhage; the whole deduced from an extensive Series of Experiments, and illustrated by 15 *Plates.* By J. F. D. Jones, M. D. Member of the Royal College of Surgeons of London. 8vo. pp. 237. London. 1805.*

[From the Edinburgh Medical and Surgical Journal for 1806.]

OUR curiosity, early attracted by the importance of the subjects announced in the title, has been amply gratified by the perusal of this interesting volume, which has left upon our minds very favourable impressions of the critical judgment of the author, as well as of his talent for experimental inquiry.

Few facts in physiology are more curious, or more interesting to the naturalist, than those which belong to the subject of animal reproduction. There are few of higher importance to the surgeon; for this knowledge points out to him, in many cases, at once the object and the limits of his art. The process employed by nature in the reproduction and reunion of separated parts are the fairest subjects of experimental inquiry, and the labours of many ingenious men have accordingly been rewarded by the deriving of new and important facts. The observations and experiments of Duhamel, Haller, Troyes, and M'Donald; of Murray, Huhn, and Hunter; of Cruickshanks and of Haighton, leave, indeed, little to desire in the departments which they have examined.

From the most perfect restoration of a lost or amputed part, to the cicatrization of the simplest wound, we observe a uniformity in the attempts of nature at reproduction, or a series of analogous events, which expound to us the process of nature in the reproduction and reunion of the bones, and of most of the soft parts of animals. The process employed by nature in suppressing the hemorrhage from divided arteries, though perhaps of all the most important to be fully understood, has not been so perfectly explained. Experiments and observa-

* Lately republished by Thomas Dobson, Philadelphia.

tions, indeed, have not been more neglected here; badly imagined, however, and limited in their direction, by the influence of some hypothesis to be confirmed or refuted, they leave us still uncertain of the truth. Some steps of the process have been seen or affirmed, while others, equally important and efficient, have been overlooked or denied. Limited observation, and the inclination to simplify the mechanism of nature, have led to theories founded on some one or other of the steps of the entire process; and hemorrhage has been supposed to be naturally stopped by an obturating clot of blood, by the contraction and crispation of the divided artery itself, or by the tumefaction or injection of the surrounding cellular substance. The limited theories of Petit, of Morand, and of Pouteau, gave rise to other subordinate hypotheses not more satisfactory. Distrusting, then, these results of partial observation and of hasty conjecture, to arrive at the truth, it was necessary again to consult nature herself; and by a series of observations and experiments, carried on through every stage of the process, from the first effusion of blood to the natural suppression of the hemorrhage, and complete cicatrization of the wounded artery, to mark the various changes which take place, and the order in which they succeed: a task which has been undertaken and executed, with equal zeal, ability, and success, by Dr. Jones.

The experiments are numerous, but not redundant; they are well imagined, skilfully executed, and, to all appearance, faithfully related. The results are also exhibited in 15 neat engravings. The experiments are compared with, and his observations are afterwards illustrated and confirmed by, those of other eminent physiologists and surgeons. For the error of his predecessors seems to have been chiefly that of seizing exclusively one step of the process of nature which they really did observe, and of hastily concluding that they had nothing farther to look for. So far, indeed, as they did observe, their observation was correct and accurate; but this being limited to some particular period of the process, they differently saw the corresponding steps of a series of changes, which really constitute the process of nature in suppressing the hemorrhage from divided or wounded arteries.

Dr. Jones having examined and freely exposed the defects

of Petit, Morand, Sharp, Pouteau, Gooch, Kirkland, and J. Bell, proceeds in the relation of a series of experiments on the arteries of horses and dogs, undertaken with the view of ascertaining the process employed by nature in the suppression of hemorrhage from *divided* arteries, and the order of the events which constitute it. In these experiments the larger arteries were completely divided; the suppression of the hemorrhage was left to nature, and the condition of the divided vessel was, in the different cases, ascertained by careful examination, at different periods, after the first cessation of the hemorrhage. Though we must refer to the original for a full detail of those experiments, we shall here quote the following as examples.

EXPERIMENT VII.

'The femoral artery of a dog was divided, and the integuments were brought together, as in Experiment II., the section of the artery being made as high as it was detached. Half an hour after the hemorrhage had completely ceased, the dog was drowned.

'Dissection. A considerable clot of blood was found between the integuments and the artery, covering both of its cut extremities, and adhering to the lower, and to the parts about it: the extremities of the artery were nearly an inch distant from each other: a black cylindrical coagulum was found stopping up the mouth of the upper extremity, and extending at least one third of an inch down from it, and between the vein and nerve. The mouth of this extremity was slightly contracted. The division of the artery appeared to have been made immediately at its connexion with the cellular membrane; this appearance was, no doubt, rendered more complete by the retraction which had taken place. There was an effusion of blood between the artery and its sheath, to the extent of at least two inches: there was also a considerable effusion in the surrounding cellular membrane; but the artery had not the slightest appearance of being compressed by it. On cutting open this part of the vessel, a long and very slender coagulum of blood was found within it, which by no means filled up its canal at any part, nor adhered to the internal coat of the artery. Hereafter I shall call this the internal coagulum, to distinguish it from the external.

About four lines breadth of the inferior portion of the divided artery was detached from the surrounding cellular membrane; its mouth was much more contracted than the upper, and was slightly turned on one side; it adhered to the clot, which filled the wound, and lay over it; and the internal coagulum was very slender and thready.

EXPERIMENT XVII.

'The carotid artery of a horse was divided just above a ligature, which had been made on it to prevent hemorrhage from that portion of it next the heart, and the integuments were secured by sutures previously passed. The blood flowed too fast at two or three interstices, but these were closed by additional sutures, and the external hemorrhage presently ceased. A very large tumor instantly formed, but its size considerably diminished in the course of twenty-four hours. The animal was killed sixty-six hours after the operation.

'Dissection. The clot, which originally filled the cavity of the wound, and distended the integuments, had nearly disappeared, having been either washed away by the discharge or absorbed. The ends of the artery were separated between one and two inches. The sheath was tinged with blood to the extent of many inches. To the circumference of the cut artery, and just within it, the external coagulum, consisting partly of lymph, partly of blood, adhered. Its figure was conical, and it was supported at the mouth of the artery by its intimate connexion with the inner lamina of the sheath, which, by the retraction of the artery, formed a canal for it; and it derived farther support on all sides, from the blood effused and coagulated between the inner and outermost lamina of the sheath. The internal coagulum was an inch and a half long, corresponding to the distance between the external coagulum and the first collateral branch. It completely filled the canal of the artery, and had every appearance of having been formed soon after the operation. It was quite detached, and lay two inches above the external coagulum, having, in all probability, slipped from its original situation in handling the parts previous to the artery being opened. See plate II. fig. 2.'

EXPERIMENT XIX.

'The femoral artery of a dog was divided, and the integuments were brought together in the manner already described. The animal was killed nine days after the operation.

'Dissection. The wound was open, but its extent much diminished. Its surface was formed of a thick layer of very vascular lymph, which being divided, discovered the truncated extremities of the artery half an inch apart. The cellular membrane surrounding each extremity of the artery, for the space of an inch, was very much thickened with coagulated lymph. The superior portion of the artery was slightly contracted at its extremity, which was completely closed, and filled up with lymph. From this closed extremity extended, about two lines breadth, a small rounded whitish substance, of the consistence of jelly, which, probably, was the remains of the external coagulum not yet absorbed. Within this portion of artery we found a small conical coagulum of blood, attached at its base to the lymph that closed the mouth of the artery, but not adhering to, nor even appearing to touch, any other point of its internal surface.

'The inferior extremity of the artery was much more contracted than the superior, its termination being very distinctly of the figure of a cone. On cutting it open, we found its mouth completely contracted, and adhering to the lymph that closed it. An internal coagulum, similar to that of the superior portion of the artery, was attached to this lymph. The coats of both portions of the artery were very much thickened. See plate I. fig. 4.'

Not one, then, but a variety of circumstances conspires in the natural suppression of the hemorrhage from *divided* arteries. The divided artery retracts; and contracts the force of the circulation, after the first impetuous flow of blood is gradually weakened and reduced; the blood is effused into the cellular substance, and the sheath within which the divided artery had retracted; the effused blood is here entangled, and the foundation laid for the formation of a coagulum, which fills the sheath and cellular membrane, and eventually closes up the

mouth of the artery; and this, which is termed the external coagulum, is the first complete barrier to the effusion of blood. "This coagulum, viewed externally, appears like a continuation of the artery; but, on cutting up the artery, its termination can be distinctly seen with the coagulum completely shutting up its mouth, and enclosed in its sheath."

The next step in the process is the formation of the *internal* coagulum, the clot within the artery, a slender conical clot which lies loose in the arterial canal, and connected with the artery only by its base, which, by its circumference, is slightly attached to the divided extremity of the vessel. The formation of this internal coagulum, however, appears to be merely a contingent event in the process, and depends on the cessation of the circulation in that part of the artery which lies between the first collateral branch and the divided extremity, after that extremity has been closed, and the hemorrhage stopped by the external coagulum. The figure and size of the internal coagulum vary according to the remoteness of the first collateral branches, and where this goes off very near to the divided extremity of the artery, the internal clot is often not to be found. "The *internal* coagulum contributes nothing to the suppression of hemorrhage in ordinary accidents, because its formation is uncertain, or, when formed, it rarely fills the canal of the artery, or, if it fills the canal, does not adhere to the internal coat of the artery."

Soon after, there is observed, between the external and internal coagula, a layer of coagulable lymph, poured out by the inflamed vessels of the cut extremity of the artery, to the internal coat of which this coagulum of lymph is firmly united. And now, by the gradual contraction of the artery, and by the effusion of lymph, these parts become intimately blended together; the canal of the artery is obliterated, and its extremity lost in the surrounding parts. Thus, the *temporary* suppression of the hemorrhage is accomplished by the retraction and contraction of the artery, and by the formation of the coagula, or clots of blood; *permanent* security is afterwards obtained by the effusion, consolidation, and organization of coagulable lymph. The artery, however, gradually undergoes other changes: "Its obliterated extremity no longer allowing the blood to circulate

through it, the portion which lies between it and the first lateral branch is no more distended and excited to action as formerly, but gradually contracts, till at length its cavity is entirely obliterated, and its condensed tunics assume a ligamentous appearance." "At the same time, the remarkable appearances at the extremity of the artery are undergoing a considerable change; the external coagulum of blood, which, in the first instance, had stopped the hemorrhage, is absorbed in the course of a few days, and the coagulating lymph which had been effused around it, and had produced a thickened and almost cartilaginous appearance in the parts, is gradually removed, and they again appear more or less completely restored to their cellular texture."

Such is the outline of the process employed by nature for the suppression of hemorrhage from *divided* arteries, as more fully deduced by our author from his own experiments and observations, and which he very happily illustrates and confirms by a judicious exposition and criticism of some of the observations of Pouteau, Kirkland, Morand, Gooch, Haller, and others.

Let us conclude, then, with Dr. Jones, that "we can no longer consider the suppression of hemorrhage as a simple, a mere mechanical effect, but as a process prepared by the concurrent and successive operation of many causes. These may be briefly stated to consist in the retraction and contraction of the artery, the formation of a coagulum at its mouth, the inflammation and consolidation of its extremity by an effusion of coagulable lymph within its canal, between its tunics, and in the cellular substance surrounding it."

We now pass to the 2d chapter of this treatise:—"On the means which nature employs for suppressing hemorrhage from *punctured* or *partially divided* arteries; and on the process of reparation which takes place in those arteries."

The common consequence of a punctured artery, in man at least, is the formation of aneurism; and the experiments of this chapter were originally instituted with the view of ascertaining the manner in which aneurism is produced, and with little hope of witnessing the complete and perfect reunion of a partially wounded artery. But Dr. Jones found it very difficult to

produce aneurisms in the arteries of horses and dogs; on the contrary, he discovered that, when the artery was simply punctured, the wound often cicatrized by a process of reparation, its canal continuing pervious, and its functions entire; or, when a larger portion of the circumference of the artery was wounded, that either the canal of the artery became obstructed, or that a complete division took place by laceration or ulceration.

We shall take the 6th of this series of experiments as an example of the cicatrization of a punctured artery.

EXPERIMENT VI.

"The carotid artery of a dog was laid bare, and a longitudinal wound made in it with a lancet, without removing it at all from its situation or surrounding attachments; a profuse hemorrhage followed; the integuments were sewed up as quickly as possible, and soon after they were found distended with blood, and the hemorrhage ceased.

"Nine days after this experiment the animal was killed, and, on examining the parts, the external wound was found to be very nearly healed. Its surface was formed by a vascular layer of lymph. The artery was injected from the aorta, and the injection passed very readily through it. As it had been wounded anteriorly, I cut open its posterior part, immediately opposite to the wound. The canal of the artery and the injection were very slightly narrowed just at this part; the coats of the artery and the surrounding cellular membrane were very much thickened. On picking away the portion of injection which passed through this part of the artery, the longitudinal wound was seen to be completely cicatrized. There was a collateral branch filled with injection on one side of it, and on the other a very thin lamina of lymph, adhering to the internal surface of the artery. See plate V. fig. 3."

But if those animals had been longer preserved, if they had fully recovered their blood and health, if they had been allowed to return to their wonted freedom and exercise, might not these cicatrized arteries have dilated into aneurisms? However this may be, the process of cicatrization, as deduced from these experiments, appears to be this:—The sheath becomes injected with blood; the relative position of the puncture in the

artery and in the sheath is altered, so that they no longer oppose each other; a layer of coagulated blood is confined by the sheath over the puncture in the artery, and forms the *temporary* barrier to farther hemorrhage. Lymph is now effused under the coagulum of blood, and the process of reparation is completed in the usual way, till permanent security is obtained.

Though it appears, from Dr. Jones's experiments, that the punctured arteries of brute animals may thus be cicatrized, and their functions preserved entire, we are not to flatter ourselves with equal success in the surgery of wounded arteries in man. Although there is such an appearance of cicatrix in a case quoted from Petit, we perfectly agree with Dr. Jones, that in the treatment of a wounded artery, "in every case in which it can be done, it is best to tie the artery above and below the wounded part, and to divide it between the ligatures."

Chapter 3:—"On the operation of the ligature; showing that its immediate effect is to divide the middle and internal coats of an artery which gives rise to the adhesive inflammation."

The experiments contained under this head are novel, and highly interesting. In these, after exposing the arteries, ligatures were passed round them, and tied in the usual way, but, immediately afterwards, loosened and withdrawn; the freedom of circulation was instantly restored, and the blood passed through the artery as before the application of the ligature. Yet, very shortly after, the artery became obstructed, and was eventually cicatrized for some way above and below where the ligature had operated, and that as effectually as if the ligature had been suffered to remain. The process by which this is brought about seems to consist of the following parts: The internal and middle coats of the artery are torn or divided by the ligature, an observation first made by Dessault, and confirmed by Mr. Thomson and by Dr. Jones: the divided coats inflame; coagulable lymph is poured out so abundantly as to obstruct the arterial canal; above and below this obstructing coagulated lymph there are formed internal clots, or coagula of blood, as far as the first collateral branches, which complete the obstruction, and at length all this portion of the artery becomes cicatrized, with the circumstances more fully exposed by our author in the next chapter, "On the process of adhe-

sion, and the changes which an artery finally undergoes, in consequence of the application of the ligature."

In the experiments undertaken with a view to the investigation of this process, the ligatures were applied in the usual manner, and allowed to remain, the artery being, in some, divided between two ligatures, and allowed to retract, and, in other cases, left undivided.

From the whole of Dr. Jones's experiments, it appears that the effects of tying an artery properly are,

1st, To cut through the internal and middle coats of the artery, and to bring the wounded surfaces into perfect opposition.

2d, To occasion a determination of blood on the collateral branches.

3d, To allow of the formation of a coagulum of blood just within the artery, provided a collateral branch is not very near the ligature.

4th, To excite inflammation on the internal and middle coats of the artery by having cut them through, and consequently to give rise to an effusion of lymph, by which the wounded surfaces are united, and the canal is rendered impervious; to produce a simultaneous inflammation on the corresponding external surface of the artery, by which it becomes very much thickened with effused lymph; and, at the same time, from the exposure and inevitable wounding of the surrounding parts, to occasion inflammation in them, and an effusion of lymph, which covers the artery, and forms the surface of the wound.

5th, To produce ulceration in the part of the artery round which the ligature is immediately applied, viz. its external coat.

6th, To produce indirectly a complete obliteration not only of the canal of the artery, but even of the artery itself, to the collateral branches on both sides of the part which has been tied.

7th, To give rise to an enlargement of the collateral branches.

A knowledge of the changes which an artery undergoes, in consequence of the application of the ligature, explains to us,

at the same time, its occasional failure, and instructs us how to avoid some of the causes of secondary hemorrhage; those, at least, which depend on the improper form and application of the ligature. And, with some very pertinent remarks on this subject Dr. Jones brings his treatise to a close.

It is proved by some experiments of Dr. Jones, that, to produce those changes in the artery which terminate in the adhesion of its coats and obliteration of its canal, which it is our object to attain by the proper application of the ligature, it is necessary that the internal and middle coats of the artery be completely divided by the ligature; and hence that form and mode of application, which are best calculated to produce this requisite division of the internal coats, must be preferred. Large flat ligatures are therefore improper; round ligatures, which are small and sufficiently firm, are preferable; they should be perfectly regular. No part ought to be included in the ligature but the artery. Care should be taken to tie the ligature with sufficient force, and always as nearly as possible in a direction perpendicular to the axis of the artery. The artery itself should, in every case, be as little as possible detached from the neighbouring parts: and, if experience has discovered any advantage in the mode of tying and dividing the artery between two ligatures, Dr. Jones is disposed to think that this advantage consists in the artery being tied close to the part at which its connexion with the surrounding cellular membrane is complete; whereas, when a single ligature is used, a considerable portion of the artery is detached, and the ligature, perhaps, applied in the centre; or, if applied at the upper end, still there remains a considerable portion of detached artery below it.

From the foregoing analysis, our readers will be enabled to anticipate the general merits of this excellent treatise. To us it appears a work of uncommon merit, and we doubt not our judgment will be confirmed by every one who, after an attentive perusal, considers the unusual labour bestowed upon it, the great number of facts contained in it, the excellence of the plan by which all those facts are arranged, the precise and accurate development of the most important processes, of which very inadequate and confused notions had been formerly en-

tertained, and the industry and fidelity with which they have been illustrated by the scattered facts relating to these collected from the writings of others.

Practical Observations on Cancer. By the late John Howard, *Fellow of the Royal College of Surgeons, and Surgeon Extraordinary to the Cancer-Ward in the Middlesex Hospital. London*, 1811, *pp*. 144.

[From the New Medical and Physical Journal for Dec. 1811.]

SO little has hitherto been accomplished towards the attainment of a complete knowledge of the nature of cancer, and the treatment of this disease is still so far from being tolerably successful, that a wide field is yet open for the labours of the pathologist and the experiment of the practitioner; every attempt, therefore, to facilitate our progress in this important inquiry cannot but be received with satisfaction by those who have at heart the advancement of the healing art, and the alleviation of human suffering. The opinions and experience of a surgeon much occupied in the treatment of this disease cannot fail of being interesting to his professional brethren; and although the author may not have thrown much additional light on the nature of cancer, we conceive he has afforded many important hints, calculated to induce surgeons to adopt a judicious and improved method of treatment. A very important step, previous to the investigation of the causes and progress of cancer, is, to distinguish it from other diseases to which it bears resemblance, or with which it may be confounded. Our author has attempted to do this, not by giving a formal definition of the disease in a few words, but by describing the various circumstances under which the disease most frequently occurs, the temperament or constitution, the age and habit with which it is generally accompanied. After stating that cancer is originally a disease of a gland or glands, and demonstrating that the glandular system occupies a very extensive and important part in the animal economy, the author enters into an inquiry, whether a certain relation does not exist between scrofula and cancer; the appearance of disease in the one form or the other

depending upon the age, or other circumstances of the individual.

"From the most careful observations I have been able to make, for many years, it has appeared that a number of the most deplorable cases of cancer, towards the decline of life, have been in habits apparently scrofulous, from the complexion and from other circumstances. It has been admitted, that scrofula in infancy, childhood, and during the progress of life, towards puberty, is characterized by different appearances from those of more advanced age. Not that I mean to say that cancer is a disease strictly speaking of the lymphatic glands; for it is generally an affection of other glands of a very different kind, and destined to a very different purpose in the animal economy. But it is nevertheless probable that the lymphatic system, if not the lymphatic glands themselves, at a late period of life, may have an immediate and near connexion with this disease. We know well that a wonderful attraction may take place from age, not only in the nerves, arteries, and veins of the human body, but in every gland throughout the body, and in every viscus in it. Is it therefore probable that the lymphatics are the only organized parts exempted from this general change? Scrofula has been supposed, at least early in life, to be a disease of the lymphatic system and lymphatic glands. The disease, after puberty, and during the state of manhood, and also during the middle state of life, in men and women, may no longer manifest the same character. The original character may be blended with other diseases in the system, as cough, consumption, obstruction of the mesenteric glands, &c. But later in life, when alterations are daily taking place in the habit, and in its general economy, the lymphatic glands, in common with all other glands of the body, undergo remarkable changes. The lymphatic vessels themselves are connected with every gland of the body, and they are as essential to man as nerves, arteries, and veins; these therefore may be influenced by time, accident, or age, and may also undergo a change; and that of a morbid kind. It has been generally supposed that scrofula, as has been stated, is a disease most prevalent in the more early part of life; if it be admitted that it may remain quiet for years, may it not afterwards assume, in the same

person, new forms? To enter into the variety of forms which this disease may assume, would be foreign to my purpose; but from the prevalence of cancer in scrofulous habits late in life, I am convinced that this is a common termination of it.

"Scrofula has been long believed, by intelligent men, to be frequently an hereditary disease: that from some wonderful coincidences it may lie dormant in one child and be active in another; and that it may lie dormant, for a generation even, and revive in another. If these be facts, why may it not also be inactive in the same individual during the middle period of life, after having manifested its character in infancy, and on to puberty? and why may it not return towards the decline of life in a new form—in the form of cancer? Cancers of the throat and tongue, I suspect, have thus originated; and cancers of the uterus, and of the breasts of women at an advanced period, I have great reason to believe, in many instances, partake of the same leaven."

A similar connexion is also said to exist between cancer and lepra, elephantiasis, and hereditary diseases of the skin; or that these latter may prove either predisponent, or exciting causes of cancer; but this proposition is by no means clearly established. Indeed, in treating of the causes of cancer, we think the author is particularly obscure and confused; nor do we conceive that any satisfactory information can be derived from what is here laid down on the subject.

"There seems to be a predisponent, an exciting, and a proximate cause in the production of every cancer. Thus, a blow on a glandular part may be the predisponent cause, but the exciting cause may be whatever is capable of putting the first cause into action and further motion. If I take a number of persons, and suppose them all to have received injury by the predisponent cause above mentioned, the particular habits of these persons may come in as auxiliaries to carry on the effects of the blow. Here then an hereditary disposition to cancer, a tendency to lepra, to cutaneous diseases, or what are vulgarly called humours, by a kind of metastasis to the injured gland, may become exciting causes. But when no such accident has happened, these are of primary consideration, and may be predisponent to other cancers: and we must look elsewhere for

the exciting cause. We know that in women, as age advances to a certain period, a remarkable alteration of structure takes place in glandular parts; those in the breast, which were destined for the secretion of milk, become useless; and the uterine glands are no longer influenced by menstruation; added to which, other changes do frequently take place at the same period in the whole system, or general economy of the body; and these incline many women to erysipelas, gout, rheumatism, asthma, and to congestions in various parts, but more particularly in the legs. Any of these different affections may be the exciting cause; thus, what were after a blow exciting causes, may be predisponent under different circumstances, where no such accident has ever happened; and in this light I must consider an hereditary disposition to cancer, a tendency to lepra, to cutaneous diseases, or what have been termed humours, as predisponent or exciting. In such cases, the co-operation of the predisponent, with the exciting cause produces a third cause which may be called the proximate in the formation of a scirrhus, which I take to be the first germ, or beginning of cancer." It is surely contrary to the generally received notion of causes, to call a blow the predisponent cause, and the previous state of the system the exciting cause; it is the state of the system which commonly determines whether this or that disease shall be produced by an accidental injury, which is the cause that actually excites the disease into action; the hereditary disposition to cancer, instead of being, as the author says, the exciting cause is clearly the predisponent, without which hereditary disposition, the accidental blow, his predisponent cause, would only have produced simple phlegmon.

Among the causes of cancer, animalculæ have been reckoned: but Mr. Howard does not seem disposed to admit them into the list; they have been found to exist in the human subject without producing cancer. Neither is the author satisfied that hydatids have any connexion with cancer; in one operation, he saw an hydatid appearance of the cellular membrane, during the removal of the breast; the knife having made a division of soft, not of glandular, parts, which looked much like hydatids, when cut transversely. This circumstance was considered by the author as a peculiar mark of the disease having extended

to the cellular membrane. He never recollected having seen such an appearance before or since. "If the cause of some cancers," says the author, "be a virus, I suspect it is not strictly spesking sui generis, as the small-pox, but an affection of the leprous, or of the elephantiasis kind; and this may, in some measure, account for the great frequency of its repullulation." It is not evident how the frequent repullulation of the disease can be more easily explained by supposing the cancerous virus to partake of the nature of lepra, or elephantiasis, than if the cause of the disease were, as is probably the case, a virus *sui generis*. In either case, while the predisposition existed in the system, the disease would be easily reproduced by the application of the exciting causes.

Of the progress of the disease from its state of scirrhus, which perhaps at first may be confined to a single miliary gland of the skin, to extensive ulceration, the author has given a just description; the following observations, we think, claim considerable attention.

"The tumour having increased, or grown to such a size, that the skin can no longer bear its pressing forwards, it gives way, and the cancer is then ulcerating, or already ulcerated. When ulcerated, it may spread either slowly or rapidly, and the sore may be small or large. There is, however, a circumstance I have sometimes seen, which is not an immediate and direct ulceration (I mean not a deep one), but an exudation from the pores of the skin, followed either by an excoriation, or scab, or both. This scab, or crust, defends the part for a time from further ulceration, and seems to check the growth, in a measure, in some, and alter the figure of the tumour in other cases; and I have sometimes seen it tuck down in the middle of the tumour, having a thick, reddened, hardened circumference, like a wall round it, approaching sometimes to a figure nearly circular. To the formation of this scab I must desire the reader to attend particularly. It may be considered as the work of nature to relieve herself, which gives some respite to the patient from the more distressing ravages of the disease; and it is for a time, at least, a defence from irritation and further ulceration. I have known it remain for months and years; and it supersedes, in some measure, every topical application.

This, however, can only last for a period; and if it be picked off, or otherwise irritated, rapid ulceration will probably follow. But it may happen, in some cases, to fall off, and to be renewed from time to time without much increase of irritation. At first it is hard, and as was before said, crusty; and this crust may increase as the discharge increases and accumulates; it softens however by degrees, and at length becomes a slough. Let not this appear a trifling circumstance; for the prolongation of life depends on this scab, in as far as it checks the progress of the disease to ulceration; which latter, when once established, is sure to lead to fatal consequences."

However small may be the original tumor, it seldom fails to extend its ravages to the neighbouring glands; the quick progress of the disease, the author thinks, may depend on a glandular sympathy or attraction; or, he says, the tubercle or gland may be compared to the bulbous root of a plant in the earth; or to a fungus or toad-stool on the bark of a tree, which vegetates in every direction; for fungus excrescences are common on the bark of the oak, the larch, and on other trees. What is an ordinary wart, but an excrescence growing in the skin, and common integuments? These, though not cancerous, are species of morbid animal vegetation. The author attributes, in some measure, the return of the disease, after the operation, to some of the younger semina or roots having been left, which, in time, will carry on the propagation of the disease, although the great original mass of mischief be extirpated. So far as the return of the disease is effected in this manner, there does not require the presence of leprous or elephantiasis virus to explain its reproduction, it must equally take place if the cancerous virus is one *sui generis*.

Cancer may affect various parts of the body, and the symptoms and progress will vary according to the part affected. The more slow or rapid progress of the disease, the author thinks, is also influenced by the temperament of the individual; the rapid progress of scirrhous tumours is more particularly remarkable in fair women of a sanguineous temperament, because such are constitutionally subject to inflammation. The disease frequently attacks different individuals in the same family, indicating therefore an hereditary predisposition to it;

while some members of the same family are affected with scrofula, others with lepra or elephantiasis, and others with cancer; whence the author judges, that, in such circumstances, there is a combination of cancer, elephantiasis, and scrofula, in the same family.

Whether the absorption of cancerous matter ever takes place previous to ulceration, is a matter of considerable doubt; nor does the author attempt to determine the question: that absorption does take place after ulceration, is clearly ascertained; the propagation of the disease during its scirrhous state, is attributed by the author to *irritation*, extending from one gland to another, which, even before ulceration, may have occupied so great an extent as to preclude all hopes from an operation. Every tumour, it is observed, apparently of a scirrhous kind, may not be cancerous; but every cancer in its incipient state is truly a scirrhus: this is a distinction of great importance in practice; we have, however, no absolute mark to distinguish, in the most early state, *in all cases*, those scirrhi which are not, from those which are cancerous. The age and temperament, and other circumstances, must be taken into consideration in forming a diagnosis. As a general practical rule, the author thinks it safer to anticipate irritation and diseased action (which, sooner or later, take place in every such gland), when it is *very small and perfectly moveable*, than to suffer it to remain liable to increase, and become irritable from a mere process of nature, as well as from accidents. At the same time he admits, that even a true scirrhus may oftentimes remain quiet for months, and even years. Probably, during this its state of quiescence, it would be advisable to make trial of means to discuss the tumor, which, if not cancerous, might speedily yield; and, in case of its being really cancerous and not yielding, nothing would be lost by the trial; the operation might be had recourse to whenever the tumor should exhibit signs of increase. This method the author has in several instances adopted, and with considerable success. Indeed he has illustrated his opinions by a great number of cases, many of them very interesting and instructive. His directions for the management of scirrhi, in the early stages of the disease, are judicious and important. We shall state some of the most material in the author's own words

"Whatever means are employed to keep quiet a gland diseased from a blow or bruise, in cases where an operation will not be submitted to, the rule of practice of endeavouring to discuss it, or prevent its increase, at an *early period*, holds good. From the time of the accident, it should never be neglected, or left to itself. Applications of a sedative and discutient kind should be had recourse to immediately. Pressure from the stays should be guarded against; the bowels should be kept open, and a cooling regimen enjoined; and it may be necessary to draw blood repeatedly by means of leeches. It is before ulceration takes place, that leeches may be applied, at some distance from the injured part, and afterwards to the part itself. The action of discutients in promoting the absorption of blood extravasated from bruises, is generally quick. If therefore the above means should fail, either to give ease or to remove any hardness that may remain, blisters may be repeatedly applied, which, by stimulating the skin, and producing a considerable serous discharge, will probably prevent the injured part from taking on a diseased action. And where the injury has been neglected, and where at a distant period pain, enlargement, or preternatural fullness has taken place, the like means may be had recourse to, *spatiis catameniorum intermediis*. And, with the same precaution, a warm sea-bath, or even the common warm bath may be employed. The immersion of the body draws the circulation and nervous power to the whole surface of the skin, and increases the sensible and insensible perspiration from the cutaneous glands and pores; it relaxes not only the injured parts, but the system in general; and thus, I conceive, relieves by causing a powerful resolution; but it is very probable that sea-water, as well as some mineral warm baths, the Harrowgate for instance, may have a specific action independent of their operation as warm water."

The author adds, "I am strongly inclined to believe, that a judicious and careful trial of the above plans, when the complaint is not proceeding with rapidity and provided there be time for such treatment, will render even the operation itself more successful should it be resorted to afterwards."

Whatever means are employed, it is of the last importance, as has been before said, that they should be adopted in a very

early stage of the complaint; and so sanguine is the author in his expectations, that he is almost led to believe, that, "*if external and internal means of relief are applied with due discrimination and judgment, sufficiently early, the knife even may be superseded.*" The external applications recommended by the author, in addition to leeches and blisters, are chiefly of the sedative kind; such as the preparations of lead, with which opium may be combined. The nutritum and unguentum tripharmacum of the old dispensatories are highly spoken of by the author; and he has experienced very good effects from a solution of the muriate of ammonia in water. Gentle laxative medicines are to be given internally, and continued for a considerable time; the diet is to be cooling and light, chiefly of the vegetable kind; all stimulants and irritating causes are to be carefully avoided. The author's experience confirms what is but too readily acknowledged, the frequent failure of an operation to effect a cure, even when performed under the most favourable circumstances.

The following interesting case shall close this article.

"Case 43. A married lady, of a thin habit of body, and scrofulous, had one breast removed for a cancer. Within seven months the disease returned, at the cicatrix, and a new tumour arose, but so circumscribed that, in consultation it was determined, the most probable chance for life would be to remove this tumour also. In a short time after the wound of this second operation had healed, a small immovuable hardness arose on the cicatrix, and there were two other small moveable tumours, not bigger than peas, near the axilla. These last, in time, approximated, and the two became one tumour, and like that on the cicatrix, was also fixed. The progress of this last was quicker than the tumour on the cicatrix.

"Great care having been taking to keep the skin whole, by defending it with a mild litharge plaster, an *exudation* only took place for a considerable time, through its pores; that was followed by excoriation, by a yellowish crust or scab, and at length by ulceration. The tumour on the cicatrix followed now the course of the last tumour, and it ulcerated also. There was only an interval of *seven months* from the removal of the breast until the second operation; but there was an interval of nearly

the same number of *years* from the latter to the final termination of the disease. Under the idea that hemlock internally, and Plummer's alterative pills might be of service, she gave a fair trial to both for a considerable time. From the hemlock she derived no advantage. Plummer's alterative mercurial was taken for many weeks, and towards the close of the course, it seemed to have diminished the tumour nearest the axilla; it made no impression, however, on the other tumours, and injured her general health. It rendered her irritable, disposed her to colds and coughs, and laid the foundation of repeated attacks on the chest. When I say that, I believe it did no more than accelerate the kind of pulmonic affection which takes place sooner or later in most cancers of the breast. She often went out during the use of this medicine in inclement weather, contrary to advice.

"The last mentioned tumours, though they finally ulcerated, were at no time larger than a small nutmeg. Other tumours, apparently of the same kind, afterward arose in their neighbourhood, in the skin. Their number was great, though in size small; for she was a thin spare woman. Thus the disease went on, from one gland to another, first hardening, then ulcerating, until the several ulcerations extended to the size of a large dish, reaching not only to the axilla, but to the scapula on that side. In this manner and direction the principal ravages were made, without running on to the sternum and to the other breast. By the constant use of a poultice of boiled carrots, these ulcerations had not only no fetor, but they often looked well, and would heal in one part, whilst they broke out in another. Partly, however, from the constant drain, and partly from repeated attacks on the chest, she got every day weaker, more emaciated, and hectical. Under all these distressing circumstances, the place where there was a tumour, near the axilla, showed not only a disposition to heal, but was so far healed that the tucking down of the skin made a kind of stricture on that part. This, together with other diseased glands in the axilla, produced such a pressure on the lymphatics, nerves, and bloodvessels of the arm on that side, that, from the distension and load, great numbness and pain were at times experienced. The loaded lymphatics might be traced down the arm; and, at certain distances, sup-

puration bursting forth, afforded a temporary relief. The whole extremity, even to the fingers, was not only much swelled, but the skin was in some parts discoloured, from the want of sufficient circulation. The lungs were frequently affected with an asthmatic kind of proxysms, and those affections were attended with cough, with pain in the side, and a sense of stricture and suffocation. These internal symptoms alternated with the painful state of the arm; in proportion as the one part was more violently affected, so was the other relieved; and in this way it went on to its final termination. This person had been subject for years, both before and after the attack of the disease, to violent head-ache, a very distressing vomiting, and bilious affections of the bowels. It is a very interesting case. The second tumour arising on the cicatrix so soon after the removal of the breast, shows that a portion of the disease was left behind, and produced its regeneration; and the rise of the small tumours, after the second operation, points out that there were germs remaining, which were the occasion of others being multiplied to such an extent that the disease ended in death. The whole progress of the tumours demonstrate, in my opinion, a *local glandular sympathy*, and seems to show that there was no absorption of matter into the system from the first and second tumours, nor from the three last before ulceration; and if so, the disease, whilst in such states, may be said to be *local*. If the disease be propagated by a germ, or one diseased gland, it may be like a seed planted in the grǿund, having a local action where sown, and extending only to a certain space, without influencing the whole of the system."

Observations sur la Nature et le Traitment de la Phthisie Pulmonaire. Par Antoine Portal, Professeur de Médecine au Collège de France, &c. Edition revue et augmentée par l'Auteur. Avec des Observations et des Remarques par M. Murhy, Docteur en Médecine à Hanovre, &c. qui a traduit cet Ouvrage en Allemand, et avec celles de M. Gaspard Fèdèrigo, Médecin Practisien de Venise, &c. qu'il a traduit en Italien. 2 Tom. 8vo. Paris, 1809.

[From the London Medical Review for January, 1811.]

THE author of these elaborate observations on pulmonary consumption, has been advantageously known to the profession in the different countries of Europe for nearly half a century. He deservedly acquired much reputation for his anatomical abilities, both as a teacher and as a writer; he was moreover the coadjutor of the celebrated Lieutaud in the "Historia Anatomico Medica;" he also enriched the annals of the Academy of Sciences, of which he was a member, with many valuable contributions in a professional capacity. After this respectable institution with all others was suppressed in the beginning of the revolution, having lost his colleagues who withdrew into the country, and his pupils leaving him, for it was now no longer safe to teach, he filled up his leisure in selecting from his notes the remarks that he had made on phthisis pulmonalis, to which disease his attention had been more particularly directed by having been threatened with this complaint. Thus originated these "Observations;" the first edition of which was published in one volume, in the year 1792. The second edition the author has illustrated with the notes of Dr. Murhy, professor of the practice of physic at Hanover, and Dr. Gaspar Federigo, physician at Venice, two of the translators of this valuable performance.

We sit down with no small interest to give the public some account of these volumes, in as much as we consider the subject important almost beyond any other; and because this is the work of a veteran who has laboured with uncommon diligence to establish by the knife an accurate diagnosis. We shall make several extracts from the work, and leave the reader to form his

own judgment of their merit: first, stating the manner in which the author has divided his subject. After a preface and introduction by himself, and a preface by Mr. Murhy, he divides phthisis into fourteen species.

1. The scrofulous phthisis, which he subdivides into hereditary and accidental.
2. The plethoric phthisis.
3. The phthisis that follows inflammation of the lungs.
4. Phthisis that succeeds to eruptive fevers, cutaneous diseases, and metastases arising from them.
5. Catarrhal phthisis.
6. Phthisis succeeding asthma.
7. Arthritic and rheumatic phthisis.
8. On concretions found in the respiratory passages; and on calculous phthisis.
9. Scorbutic phthisis.
10. Syphilitic phthisis.
11. Phthisis succeeding fever.
12. Nervous, hypochondriac and hysteric phthisis.
13. Phthisis the consequence of delivery.
14. Observations on phthisis succeeding contusions and wounds of the breast.

Some general observations on the symptoms of each species, on the duration of the disease, and on the blood of phthisical patients, are then offered. Observations on the dissections, and on the general treatment follow. The work concludes with some reflections on the communication of the lungs with the arm and external parts of the breast. There is also annexed a copious analytical index.

Section first, p. 1.

Scrofulous Phthisis.

"There are two sorts of this phthisis: one hereditary, or acquired in nursing; the other may be induced at any time by occasional causes."

We shall give an instance of that kind of phthisis which is derived from the nurse.

"Dissection 1st. I opened the bodies of three young children belonging to M. Bellinger, counsellor of state, who died

of phthisis; one of them did not spit blood. I found the lungs full of concretions, some of which were corroded, and others fungous; some had the appearance of steatomatous tumours, others were scirrhous. Some were entirely suppurated, the pus that flowed from them, was whitish and clotted; of these a great many were situated in the cellular membrane of the lungs. As to the bronchial bodies,* some appeared in a sound state, others were changed, and they were in the vicinity of glands and lymphatic vessels, which leave me without any doubt that the lymphatic passages are the true seats of the disease. The glands of the mesentery and the upper part of the sides of the neck near the jugular veins, and the glands of the œsophagus were filled with tumours, containing steatomatous matter: the gums and the velum palati were not diseased. The teeth were of shining white. The spine of one of the children was enlarged, and some of the vertebræ were become soft. Mr. Bellenger, who afterwards lost two other children by the same complaint, died of apoplexy and his wife of the dropsy."

Observation 6th, p. 27.

"In the year 1779, I was called by one of my pupils to be present at the opening of the body of a child of three years of age, belonging to M. Rossignols, dealer in iron and copper-wares. He had all the symptoms of phthisis; and his mother died of the same complaint about a year before. In the lungs of this child I observed tubercles, some of which were beginning to suppurate, others were in a state of complete suppuration; some were white and full of a plastry matter. There was an effusion of water in the chest: the mesentery was full of steatomatous concretions. The teeth were of a shining white. The extremities of the clavicles next the sternum were enlarged: the spine was a little awry; its spinous processes, particularly of the back were also enlarged."

In this place, vol. I. p. 42, the question is discussed, whether original or hereditary phthisis be contagious. M. P. is most decidedly of opinion that the disease is not communicable; and his Italian commentator is of the same opinion, although Dr.

* These are the firm roundish white bodies (*corps bronchiques*) about the arches of the bronchia, which we shall have occasion further to notice.

Murhy tells us in a note on the same subject, that the Council of Health at Florence had published an advertisement and rules to prevent this contagion. In answer to this, the author and M. Fèdèrigo quote Anton. Cocchi and Castellani, &c. who entertained the same opinion. For our own parts, we should be glad that it were a more doubtful question: in this country, there can be little risk in declaring that many instances have occurred where the disease has been communicated, and particularly in the dysodic state of the disease.

We shall now produce a case of the accidental phthisis in a scrofulous habit. We lay greater stress on this division of the disease, as we believe it may almost comprehend every sort of tubercular consumption; for notwithstanding the many species of phthisis here treated of and found in nosological writers, yet where tubercles are not concomitant, we cannot consider them true phthisis, but only so many modifications of inflammation of the lungs; and where tubercles are present, true phthisis only acquires the names of the different occasional causes.

Observation 3d, vol. 1. p. 124.

"The countess of Neuperg, of a delicate complexion and about thirty years of age, had two children; her pregnancies and deliveries were always attended with much bodily pain. She came to Paris in the year 1782, and there experienced much sickness, having almost every sort of nervous complaint; such as frequent hiccups, involuntary sneezings, bursts of coughing, spasms, convulsive motions in the muscles of the trunk and of the extremities, difficulty of swallowing from convulsive contraction of the muscles of the pharynx; and for twelve days had not any evacuation of the bowels; at the same time she was harassed with frequent colics and sleepless nights. At last the most urgent nervous symptoms came on, requiring varied and successive courses of diluting and cooling medicines (*humectans et rafraichissans*) both in the form of drinks and of baths, which were the only means that could have succeeded.

"Opiates only increased the irritation; the weakest of which I forebore to administer. Her health improved when she became pregnant. Even then she experienced oppression of the

breast; her difficulty of breathing returned, particularly when going up stairs. Every month about the menstrual period a slight fever came on, her pulse became small, frequent, and hard, with a dry cough almost incessant, particularly in the night. She again made much use of diluents (*humectans*) and cooling medicines, both in drinks and in the form of baths; small but repeated bleedings three or four times during her gestation were found necessary. Towards the seventh month of her pregnancy her husband became involved in misfortunes, in which she feelingly participated; her mind was tortured; she lost her rest; her cough came on; she became hot and in pain from an inflammation of the throat; the axillary glands swelled and became hard. Several enlargements could be seen about the anterior part of the neck and side of the larynx. It was in this state that she brought into the world a living child, very small but well enough proportioned to give expectations of rearing him. The result of her delivery was unfortunate; the lochia were hardly red, and in very small quantity, but in fifteen or twenty days after, a considerable hemorrhage from the uterus took place, and her cough was now become very obstinate, without expectoration. Her throat was ulcerated; she had continued fever with accessions, copious nocturnal sweats, colicky pains, diarrhœa, and lastly an edema of the lower extremities. She retained her faculties to the last, as generally happens in this kind of disease.

"The following were the appearances on opening the body: 1. The brain was perfectly healthy without any accumulation in the bloodvessels. 2. The trachea, the inside of which we carefully examined, was not the least inflamed. The larynx and pharynx with their internal membranes were equally sound. The lungs were suffused (*infiltré*) with a reddish fluid of a bloody appearance. We found in the cellular membrane several whitish concretions, some of which were of a more solid texture; many were in a state of true suppuration. In different parts of the lungs abscesses were found, some of which were insulated, others communicated with one another. The most considerable of these abscesses were situated at the top of the left side of the lungs near the œsophagus, to which it adhered by means of a firm part of the cellular membrane. Lastly the lungs ad-

hered to the pleura and to the diaphragm. 3. The heart was in its natural state. 4. The abdominal viscera were not at all changed, with the exception of the pancreas, the glandular parts of which were larger and harder than they ought to be. 5. The hinder part of the brain and spinal marrow were natural. 6. The velum pendulum palati and the upper part of the pharynx were very red, and covered with vessels filled with blood. There were on the sides of the neck several lymphatic glands of which two were of the size of a small nut, and were hard, and full of steatomatous concretions, with many little sources of purulency; there was in the right cavity of the breast a considerable effusion of sanious serum, which was of the same nature with that which flowed from the lungs."

Of the bronchial bodies, (*corps bronchiques*) already noticed, the following description is given.*

"The bronchial bodies are situated round the bifurcations of the bronchiæ, to which they are united by the cellular membrane in greater or less quantity, and in general the superior bronchial bodies are a little larger than those of the lateral bifurcations of the bronchiæ. In the natural state we observe no excretory canal in these bodies. The cellular membrane with which they are covered, and which enters into their soft and lax texture, the arterial and venous vessels which give numerous ramifications to them; the lymphatic vessels and the nerves which are spread upon their immediate surface and penetrate their structure, as well as the little lymphatic glands which help to make up part of the mass of the bronchial bodies, are so many obstacles to their being discovered, if they really exist."

"I have seen as well as others at different times, the bronchiæ discoloured with a blue blackish fluid, like that which the bronchial bodies contain."

"Independently of these bronchial bodies which we find in the lungs, there are also true lymphatic glands with which many anatomists have confounded them, of a very different nature. They are not like the bronchial bodies which are always placed round the bronchiæ, but scattered without distinction

* Vol. II. p. 306. et seq.

through the substance of the lungs, principally on their external surface. I have seen some of them that were contained in the bronchial bodies, for which they might be easily mistaken, in the same way as we see lymphatics or maxillary glands encircle the parotids. The lymphatic glands of the lungs are smaller than the bronchial bodies: they are more regularly round and harder to the touch."

We confess there is much confusion in M. P.'s description of the bronchial bodies: after giving a tolerably fair view of tubercles or these bodies in the beginning of this paragraph, he immediately confounds them with lymphatic glands, and has even thought he saw a new lymphatic gland which was firmer and rounder than others usually known; and after all ends with his favourite tumefactions or obstructions (*engorgemens.*) In different parts of these observations, we have however proofs that he is acquainted with the most accurate applications of this part of pathology.

These bronchial bodies are in reality the tubercles of Baillie and other eminent anatomists. We are told that he has seen the lymphatic glands in the lungs healthy, when the bronchial bodies were changed, and *vice versa;* and adds, "To return to tubercles, which constitute hereditary or organical phthisis, I think, after the most attentive inquiry, that they are formed by the (*engorgemens*) obstructions of the lymphatic glands spread through almost every part of the lungs, also at a distance from the bronchiæ by lymphatic obstructions of the cellular membrane of the lungs, which after having become more or less enlarged and frequently in a state of suppuration; it is not therefore surprising that we find the lungs of some phthisical patients covered with tubercles."

Another extract from the fifth species, which of all others is probably the most likely to terminate in tuberculous or real phthisis, we may without exceeding our limits be allowed to make.

Catarrhal Phthisis.

"Observation 1st. I knew a woman about thirty years old, who had been very liable to rheums and catarrhs; at last fever came on with spitting of blood, together with a great difficulty

of breathing. She never would consent to be bled; her catamenia were obstructed, and she was seized with a pain of the upper part of her throat; the sound of her voice was at first sharp and then became hoarse. The patient could find no ease in any position; she breathed with equal uneasiness when she was in bed and when she sat up. And only when she held her head a little inclined towards her breast, her respiration became a little more free. Her pulse was small and frequent, and she died after being sick nearly six months, without having experienced either colliquative sweats or diarrhœa. On opening the body, the seat of the disease was found to be the larynx and the trachea. The membrane was of a red colour, and covered with concretions that almost closed a part of the aërial canal; we found two others in the larnyx that were still larger. The substance of the lungs were perfectly sound. The vessels appeared a little more filled with blood than usual, and the right auricle and ventricle were very much distended with blood."*

"Observation 2d. A man of fifty years of age, of a strong constitution, but very often subject to catarrhs and hoarseness, was seized with a continued fever. He coughed frequently, and almost entirely lost his voice. At the beginning of the disease, the expectoration was in great quantity, thick and offensive; on the 14th day of a continued fever, it changed into a tertian. The patient remained twelve days in this state, after which he had a hectic fever and cold shiverings. There was now no longer pain in the chest; the respiration was free; he could sleep with equal ease on each side; the fever diminished, and hopes were entertained of a cure, when the patient's life was suddenly terminated. There was found on the gall-bladder a concretion of the size of a pigeon's egg, without a single drop of fluid bile. The lung on the right side was full of purulent tubercles."†

The dissections are either by M. Portal, or extracted from Morgagni De Sedibus et Causis Morborum or the Historia Anatomico Medica of Lieutaud; they are mentioned by one of his friends to be about sixty-three; they are extremely exact, and must be regarded as very valuable; for though we do not find that appearances are explained in our own manner, or as

* Vol. I. p. 339. † Ib. p. 340.

we should expect from so great a pathological anatomist, we are not disappointed with the work. We have a fund of materials which may perhaps be better employed by others. M. P.'s notions were formed from the humoral pathology of Boerhaave; and his theories have been but little disturbed by intercourse with the medical literature of the day. His practice differs very little from that of the beginning of the last century. By way of example we shall quote his own words in answer to a note of Dr. Murhy, who complains of the feebleness of his practice, and his want of acquaintance with the improvements made in other countries. In reply he says,

"1. That certain juices of plants are tonic, as cresses, scurvy-grass. In place of relaxants (*relachans*) in various cases foxglove may be ordered in extract, and in the same manner wakerobin, powder of polygala, Virginian snake-root, vesicatories and moxa in cases according to circumstances. Thus French physicians do not, at least I do not, always prescribe (*elachans*) weakening or (*adoucissans*) palliative medicines in phthisis. M. Murhy has had an opportunity of seeing a sufficient proof of it. 3. Has not Mr. Murhy seen that I do not bleed except there are sufficient indications from the fullness and hardness of the pulse, which happens particularly in that sort of phthisis from plethora, and in that preceded by inflammation?

"4. Has not M. Murhy seen that I advise emetics when the tongue is more or less charged, when there are nausea and vomitings, when there are marks of the respiratory passages being oppressed with (*mucus*) phlegm in catarrhous complaints, &c.? And then are there not counter-indications that forbid emetics?

"5. That only the signs of scrofula, syphilis or scurvy determine me to order such medicines as are suited to eradicate the vicious taint, and afterwards to contravene such pulmonary diseases with a diligence proportioned to the urgency of the case.

"6. That I order diaphoretics, sudorifics, and vesicatories &c. at the very first approach of phthisis that may be produced by the disappearance of some particular eruption, or by metastasis: tell me are there any curative means that are not founded on the science of indications, drawn from symptoms even in

the several complaints that we have to contend with: otherwise medicine would be given up to conjecture, and often to the delirium of physicians.

"As to fumigations," mentioned by M. Murhy, "we have very often tried them without advantage, and frequently even to the injury of the patient, especially when we employ gases in too active a state, and fumigations even of water too hot or charged with sharp and stimulant substances. The cow-houses are not much more useful to the unfortunate consumptive patients; all we can do is to be as useful and humane as possible, and not to deprive them of all hopes of relief, for often they continue to have some expectation of a cure whilst physicians order them medicines in which they themselves appear to have confidence."*

A long extract from Dr. Gilchrist's work in recommendation of sailing and sea voyages is given by M. Portal; he also adds his testimony to that of Sydenham, in favour of exercise in phthisis: these measures cannot be too much approved, though in some cases horse exercise may be too late or too irritating. He often speaks of using the cautery, which no doubt may be useful; but we should be glad to substitute warm plasters, blisters, issues or setons, according to circumstances. We do most sincerely recommend this work to the notice of every practitioner in countries where this disease prevails, particularly in this, where we are led to believe that it is on the increase. We should have added some extracts from Drs. Murhy and Fèdèrigo, but we are compelled, on account of the length to which this article has extended, now to bring it to a conclusion.

* Vol. II. p. 428, et seq.

ORIGINAL PAPERS.

An Account of a Case of Inflammation of the Vessel, from Venesection, which terminated fatally.

By N. CHAPMAN, M. D.

In the month of March, 1810, I was requested by Mr. Francis, of this city, to visit his coachman. On inquiring into the case, I learned that three days before, while on a journey, he had been attacked in the evening with slight symptoms of pleurisy, which very readily yielded to a moderate bleeding.

The next morning, he felt for the first time, some degree of pain and tension in the arm in which he had been bled.* But the uneasiness was so trifling, and in other respects he was so well, that he continued on the journey. The exertion of driving, as might have been expected, aggravated exceedingly these affections. Such however, was his anxiety to reach home, that he studiously disguised his real situation, lest he should be left behind; and obstinately persevered, against every remonstrance, to perform his duties as coachman. But on the last day of the journey, overcome by the severity of his sufferings, he reluctantly consented to be placed in the carriage, and was in this way conveyed to the city.

My attendance on him commenced in the night, an hour or two after his arrival. Even at this early stage, the case of my patient presented a very serious aspect. His arm was swelled to perhaps twice its natural size. The pain and inflammation were excessive. By pressure, a copious stream of purulent matter issued from the orifice. I could distinctly trace the enlargement of the vein for several inches. It imparted the sensation of a hard inelastic tube enclosed under the integuments.

But these were not the only untoward circumstances incident to the case. There was also considerable pain in the left side, and a universal soreness pervaded his body. Little or no fever was indicated by the pulse. This was weak, irregular, and quick, of a contracted volume, and rather corded.

* The right arm.

It was more a *disturbed*, than a *febrile* pulse. Certainly, it evinced nothing of an inflammatory diathesis. But though apparently not much, if at all feverish, he was greatly harassed by restlessness and inquietude. The temperature of his body was unequal and fluctuating. When his attention was fixed by conversation addressed directly to him, his mind seemed perfectly rational. But otherwise, he quickly became flighty, talked incoherently, and endeavoured to get out of bed. Whatever were my doubts as to the ultimate event, I entertained none respecting the nature of the complaint, or the causes which had produced it. I was, at once, satisfied that the whole of the existing mischief was attributable to inflammation of the vein, extending probably to the heart, and to the introduction of pus into the blood. To the latter cause, I the more promptly imputed the train of nervous affections, as I had seen phenomena of the same kind induced by the injection of pus, of milk, of oil, of mucilage and other bland fluids, into the veins of different animals.

Nor could I hesitate long as to the practice to be pursued. To subdue the inflammation, and to arrest the pus in its passage to the circulation, were indications too obvious to elude the judgment.

Notwithstanding the feebleness of arterial action, I bled him to the amount of twelve ounces. I suspected a state of depression, and thought it not unlikely that the pulse might rise by depletion. Where so large a vessel was inflamed, and seemingly too, the heart itself, it certainly was not unreasonable to conjecture that the want of diffused excitement was owing to this condition of the system.* But my anticipations were not realised. The bleeding was indeed followed by no very sensible effect.†

I next enveloped completely the arm with a blister from the

* We are indebted to Syddenham for the important fact, that in many instances, there is a *depressed*, in contradistinction to an *exhausted* system, and which is to be aroused not by *stimulation*, but by *depletion*. Brown, who had his *direct* and *indirect* debility, differs from him in maintaining, that each of these states is to be relieved by stimulants properly graduated.

† The blood drawn on this occasion, had no very peculiar appearances. It did not, it is true, separate as it ordinarily does. The serum and crassamentum were commixed, as if slightly stirred together. I could detect no pus in

elbow to the shoulder,* excepting at the puncture which was covered with a small emollient poultice to facilitate the evacuation of the matter. During the night, I ordered, moreover, that he should take, at stated intervals, a solution of salts till it purged him actively.

In the morning, he appeared in some respects to be better. The blister had drawn well, and the pain and swelling were, in consequence considerably reduced. Neither was he so restless or irritable, and his mind had ceased to wander.

When at night, I again called, he was much as he had been at my preceding visit. The swelling of the arm was perhaps somewhat further abated. But in a few hours afterwards, the pain in the side, which at no period had entirely subsided, reverted with violence. It did not however *raise his pulse!*

Convinced, by a variety of considerations, that the pain proceeded from inflammation of the heart, I placed a large blister over the region of that organ. Though relieved by this application, of the pain, I had the mortification of seeing him the succeeding morning in a situation of increased danger and alarm. With a pulse weak, quick and tremulous, he was wild and distracted. I directed that he should lose six or eight ounces of blood, by cups from the neck and temples, and to have a blister put on behind each ear.

It now appeared to me to be literally of vital importance, to intercept the pus in its course to the circulation. I therefore resolved, without delay, to make use of compression, though it seemed still to be forbid by the tumefaction of the limb. A bandage and compress were accordingly applied a short distance above the puncture. But nothing material was gained by these remedies. The swelling of the arm, however, declined, and no inconvenience was experienced from the com-

the blood. There was some little size on the surface, which a hasty and prepossessed observer might have mistaken for purulent matter.

* There is nothing, I believe, in the case of an inflamed vein so efficacious as blisters. They operate promptly and decisively. When I first used them, it was at the suggestion of my own mind, or at least I had not the consciousness of their having been employed before. On reading however, the above case to Dr. Dorsey, he told me that Dr. Physick had introduced the remedy many years ago, and his confidence in it was confirmed by very ample experience.

pression. The pus continued to flow profusely. This was late in the evening. My visit the next morning found him worse. To all the bad symptoms which previously prevailed, were now added some still more inauspicious. The morbid sensibility of his body had become so exquisite that he could not bear the slightest touch, or scarcely the weight of the bed-clothes, without complaining. So sensibly alive was he to every sudden impression, that by opening or shutting the door, or walking across the room, or by a question put to him in a sharp tone of voice, or by a strong glare of light, he would be startled, and sometimes exceedingly agitated.

At this critical juncture, I resorted to the advice of my friend Dr. James, who very obligingly met me in consultation. We agreed to give the camphorated emulsion, in large doses, and to have stimulating injections repeatedly administered. But this treatment was equally unavailing. My patient progressively sunk. Low delirium, cold extremities, tremors of the nerves, and convulsive cough soon supervened. His pulse became hardly perceptible. The pupils of the eye were widely dilated, and his countenance assumed an expression uncommonly haggered, phrensied, and distressed. Desperate as I deemed the case, I did not permit my zeal, or my exertions to be relaxed. By the constant use of the most powerful excitants, such as camphor and opium, the volatile alkali, the spirits of turpentine, ether, wine, and brandy, I protracted his existence for three days longer, without having however, during this interval a single gleam of hope afforded me, by any change, of his recovery.

In reviewing the preceding narrative, the absence of febrile, and general inflammatory action, in every stage of the progress of the case, will be apt to strike us as an anomaly of no easy solution. But, it is not wholly without analogy, and it admits at least of some explanation, on the supposition, of a predominance of nervous irritation. We see in the disease excited by the bite of a rabid animal, little fever or inflammation, but vehement nervous commotions. The virus of certain serpents,*

* This is very strikingly illustrated in the case of the man bit by a rattle snake, which is recorded by Mr. Everard Home, in the Philosophical Transactions of London for 1810, and copied into the first volume of this Journal.

insects, &c. is productive of similar effects. In none of these instances is there fever. The nerves are almost exclusively the "seat and throne" of the affection. It is nevertheless, not a little mysterious that in the present case, where so much local inflammation existed, the system was not brought into sympathy. The individual too, was a subject in whom it was most likely to happen, being in the meridian of life, of a robust frame, and with a temperament ardent and sanguineous. The truth probably is, that in all cases of simple inflammation of the vein where it runs high, a sympathetic inflammatory fever is the consequence, but, if suppuration ensues, and the pus reaches the blood, then inflammation is suppressed, and the nervous system becomes assailed.*

The morning after my patient expired, I made an examination with a view of ascertaining the exact state of things. I exposed the vein from the wrist to the axilla. The external surface of the vessel was in many places inflamed, and especially above the puncture. Betwixt this, and the shoulder, matter had penetrated through the vein by four distinct sinuses, into the neighbouring cellular membrane, forming small abscesses. Two of these sinuses were high up the arm.

I dissected very carefully the parts adjacent to the wound. Directly around it, there was an abscess containing, I presume, a large spoonful of pus mixed with a dark fœtid sanies. Sphacelus had already destroyed a portion of the cellular texture.

* To speak more plainly, may not the powers of the heart, when that organ is directly attacked by inflammation, be so crippled, or paralized as no longer to be capable of supporting febrile action, or general excitement. This hypothesis derives some degree of plausibility from a case which recently occurred in our hospital. My friend, Dr. Hartshorne, who attended the case, informs me, that it was marked by symptoms very closely resembling those of my patient. The arterial system was little affected, but the nerves exceedingly so. On dissection, the heart was found in the highest degree of inflammation, and much coagulable lymph was effused. Whether these symptoms are of sufficiently uniform occurrence to warrant this inference remains to be proved. Carditis, I am aware, is very differently described by nosologists. They give to it the phenomena of violent pneumonia. The fact however is, that it is but very rarely met with as an idiopathic affection, being in a great majority of instances, the result of pneumonic inflammation extending to the heart.

Two minute twigs of the cutaneous nerve, which ran across the vein were divided, but whether by the operation of bleeding, or by the ravages of the diseased process just mentioned, I cannot confidently pronounce.

I next laid open the vein. There was inflammation more or less from a little below the elbow to the final point of my dissection. But no where was it very great. By the appearance of the coat of the vein there was, however, the amplest proof of its having existed in the highest grade. Indeed, this is abundantly shown by the formation of the sinuses, &c. Below and above the orifice for several inches, gangrene had taken place, which was bounded by an extensive erysipelatous blush, and the inner surface of the vessel, within this space, had begun to slough.

The quantity of pus in the cavity of the vein was small. It ought, indeed, to have been stated, that for two or three days prior to his death, the discharge from the orifice had gradually diminished. No disposition whatever was evinced, any where along the canal of the vein, to an adhesion of its sides. My wish to extend the dissection was frustrated. Enough, however, has been brought to light by this partial exposition, to confirm my original notions as to the nature, and causes of this series of extraordinary affections.

In reflecting on the management of the case, I have only to regret having confided too much in compression as a means of promoting adhesion. But surely I shall escape censure for adopting a remedy which had been approved by very high and concurrent authority.* I had never before an opportunity of trying compression, under such circumstances, or of seeing it employed. My conviction now is, that it will rarely succeed. It is confessedly, at all times, difficult to produce a union in tubular or fistulous ulcers, even where pressure can be used. It is yet more so, to effect a coalescence between the opposite surfaces of bloodvessels, because among other obstacles, the coagulable lymph which is the medium of attachment, must in a great measure be swept away by the circulation as fast as it is deposited. It is besides almost im-

* Hunter, Cooper, Abernethy, &c. &c. &c.

practicable to make adequate compression on any one of the veins of the arm, without interrupting the return of the blood. I confess, however, that these speculative objections to the practice have less weight with me, than the melancholy instance of its failure which I witnessed. In the case of my patient the experiment was fairly made. For five days and nights successively, I continued the compression, and with the utmost vigilance to the due regulation of it. Distrusting, therefore, the efficacy of this expedient,* if ever I should meet with a similar case, I would most unquestionably apply *a ligature to the vein*. As far as I know, this suggestion is new, and without the sanction of experience, but, at present, I am unable to perceive any reason to discourage us from the operation. The operation, however, will not often be necessary. It can only be required where suppuration has taken place, and there are grounds for the apprehension that the matter is travelling into the circulation. Cases of this sort are extremely rare. Few at least, have been recorded. I have met, in my researches, with only one, at all analogous.† They have hitherto been contemplated by writers rather as a *possible*, than an *actual* occurrence.‡

Injuries to the veins from bleeding are for the most part,

* Let it not be understood, that I have the temerity to deny that compression has never effected this end. This is not my meaning. I believe, on the contrary, that it has, and may sometimes do it. But still, I am ignorant of any case in which its efficacy has been demonstrated by subsequent dissection, or where, in other words, adhesions were shown to exist *under the point of compression*. Yet, as it is an indisputable fact, that such obstructions of the venal tube do spontaneously arise, I cannot doubt of their being also effected by this artificial contrivance. What I design to inculcate is, that these adhesions, in which ever way brought about, are of a very precarious origin, and so slow of completion, that they ought not to be depended upon in a case of such extreme urgency as an *extensively suppurating vein*.

† This case is related by Mr. John Hunter. In dissecting the arm of a man, he traced a series of adhesions along the course of the vein. But near the axilla, the vein, says he, "had taken on suppuration, beyond which adhesions had not formed, and this had given a free passage for the matter *into the circulation, of which the patient most probably died*." It is to be regretted, that a case, apparently so interesting, should be so imperfectly detailed.

‡ Mr. Abernethy, in his essay on this subject, tells us, that he had never seen a case of the vein suppurating from venesection. But he *can conceive*

attended by only a circumscribed inflammation. The suppurative process seldom ensues. Even when it happens, it is generally limited to the vicinity of the wound, through which the pus is either freely evacuated, or is lodged in the surrounding cellular texture, and the abscess thus formed, may be made by an opening to discharge externally, if, contrary to the ordinary course, it does not do it spontaneously.

Philadelphia, November 1, 1811.

A new and expeditious Mode of preparing Phosphuret of Lime.

By EDWARD CUTBUSH, M. D.

CHEMISTS, in preparing the phosphuret of lime, usually adopt the process recommended by Dr. Pearson, or that of Van Mons; in the former, a glass or porcelain tube is used, closed at one end, into which one part of phosphorus is introduced and five parts of lime, in small pieces, the lime and phosphorus being kept in separate parts of the tube. The tube is then placed horizontally among burning coals, so that the part which contains the lime, may be brought to a red heat, while the bottom of the tube, containing the phosphorus, is not exposed to the action of the fire. When the lime becomes red hot, that part of the tube which contains the phosphorus is exposed to a red heat. The phosphorus is volatilized, and passing through the hot lime, combines with it, and forms the phosphuret: a chalk stopper is also used to prevent the access of air.

one, in which it might even be proper to divide the vessel to prevent the pus from entering the circulation. With the greatest deference to the authority of this most distinguished surgeon, I cannot help giving a preference to the ligature in those cases. It has all the advantages of the operation which he prefers, and is exempt from the hemorrhage and other inconveniences which might attend it. Nor does Mr. Charles Bell, another surgeon of great eminence and experience, appear to have met with such a case. Treating, in his system of operative surgery, of suppurating veins, he says, "the danger here is *conceived* to arise from the matter formed within the veins being carried into the circulation," &c. In fine, all the writers whom I have consulted, hold the same sort of language upon this subject.

In the process by Van Mons, *carbonate* of lime is put into a small glass matrass, one third of the vessel being left empty; this is placed in a sand bath and exposed to a sufficient degree of heat to expel the carbonic acid; towards the end of the process, a third part of phosphorus is gradually introduced, care being taken to keep the lime in a red heat. The phosphorus melts, but is prevented from burning, by the remains of the carbonic acid which is disengaged from the lime. The matrass is closed with a stopper after the whole of the phosphorus has been introduced; the stopper is provided with *a valve* to let gas escape, but permitting none to enter; the fire is then to be withdrawn: when the matrass is quite cold, the phosphuret is taken out and preserved.*

The process which I propose, and which I have adopted in two instances with success, consists in putting a portion of lime, divided into small pieces, into a Hessian *crucible*, and exposing it in a common furnace, or even in a chaffing-dish of coals, until the lime becomes red hot. The quantity of phosphorus intended to be used, being dried by means of spongy paper, is then put into another *crucible*, sufficiently capacious to contain the quantity of lime to be converted into a phosphuret. Being thus arranged, the ignited lime is *removed* from the furnace and placed in the crucible containing the phosphorus, which is to be expeditiously covered with an inverted crucible sufficiently large to receive that which now holds the lime and phosphorus, care being taken to prevent the access of atmospheric air, by surrounding the edge of the inverted crucible, in contact with the brick or tile on which it rests, with some soft clay or lute: or, a *cover* might be formed to fit the *crucible, containing the lime and phosphorus*, which could be more expeditiously managed. When the contents have remained a sufficient time to become cold, a *phosphuret of lime* will be found to have been formed. In this process, the phosphorus is volatilized by the heated lime, and combines in part with it, forming a phosphuret of lime, which, when thrown into water, produces the same phenomena as that prepared according to the process of Dr. Pearson.

* Journal de Chemie.

The phosphuret made by this process is of different hues, from a dark chocolate colour to that of a pink: a portion thrown into water continued to act three hours: the water, into which it had been thrown, when filtrated, emitted an alliaceous odour, and converted vegetable blues into green, in consequence of the lime held in solution. A small portion of phosphate of lime appeared also to have been formed. The interior parts of the crucible, which was inverted over that which contained the lime and phosphorus, were found covered with a light flocculent powder of a lively red, inclining to an orange colour. Three grains of this powder, put into three drachms of distilled water, communicated no colour to it, but was perfectly transparent on being passed through a filtre; on adding the tincture of cabbage, it became red, indicating the presence of an acid, which must be considered as *a portion* of phosphorus acidified by the oxigen of the atmospheric air contained in the inverted crucible. The powder which had been thus treated, preserved its colour. Can this orange-red powder be considered the phosphuret of carbon of Proust?* It may be inflamed by friction or the application of any ignited substance.

The preceding method of making phosphuret of lime, I consider more economical and expeditious than the processes of Van Mons, and Dr. Pearson.

Philadelphia, December 14, 1811.

* Since the above was written, I have tested this powder by inflaming a portion of it in oxigen gas; a small quantity of phosphoric acid was formed, the remaining gas being passed into lime-water, on agitation, produced a turbid appearance, indicative of the formation of carbonic acid by the union of the carbon of the phosphuret with oxigen. E. C.

Biographical Sketch of Dr. Willis.

[From the Edinburgh Medical and Surgical Journal for April, 1808.]

AMONG the various plans for the improvement of medicine which may be expected to arise in those countries which have arrived at a high state of civilization, such as relate to the treatment of insanity are, unquestionably, the most important. If the progress towards an accurate knowledge of the diseases of the mind kept pace with the number of cases, the philanthropist might calmly sit by and observe with growing satisfaction the advancement of the healing art to those degrees of perfection of which it seems to be capable. But, unfortunately, extreme refinement in the modes of practice has a contrary tendency: the treatment of maniacs is too often abandoned to the keepers of houses of confinement; and the desire of making money is too apt to extinguish that cordial interest which physicians ought to feel for the welfare of their patients; while the indolent occupation of prescribing for frivolous complaints, and the glittering trappings of wealth dazzle and corrupt those who seek them. To produce, in every lunatic, associations and habits which will enable them to perform the business of their respective stations, is the only treatment that can properly be called medical, and the more widely this mode of treatment is diffused, the more successful will our practice become. With the view of making known how much can be accomplished by well-directed efforts in cases of mental derangement, we have collected the following scanty particulars of the life of a man justly celebrated for his skill and success in that line.

Francis Willis, M. A. and M. D. was born in 1717, and died in the month of December 1807, in the 90th year of his age. He was a clergyman as well as a physician, though he devoted his attention almost exclusively to the treatment of insanity. He was a remarkably active, hale, and good-looking man; and about five years before his death, he rode ninety miles on horseback in one day, to give his vote at an election. He had been indisposed some weeks; but the day before he died he was well enough to walk twice to a neighbouring vil-

lage, at the distance of nearly a mile, to visit a patient, and the next day he continued without any apparent change of health till after dinner, when he complained suddenly of being very ill, and five minutes after expired in his chair.

He was of Brazen-nose College, Oxford, M. A. 1740, B. and D. M. 1759. He has left five sons by his first wife, one of whom has long assisted him in his establishment for lunatics. Dr. Willis acquired great celebrity both in this country and throughout Europe, for his skill in curing insanity. In consequence of the fame he acquired by his attendance upon our sovereign, his assistance was sought for by the queen of Portugal; he went to Lisbon, and had that royal personage for some months under his care, but without much success. Switzerland used to send many patients, especially the town of Geneva, where the comparative opulence of the people probably contributed to furnish the doctor with more patients, and their connexion with England might render his reputation more familiar than in other parts of the continent. Persons who have been restored to their families and friends in different parts of the world, all speak with strong feelings of gratitude and respect, of the kind treatment they received, and the relief they experienced, from their amiable and benevolent physician. One elderly lady, who had been for some time under Dr. Willis's care, and recovered, though she continued subject to occasional fits of insanity, requested to reside in his house, because she was so happy in his company, and was so confident of receiving the best assistance in case she had any relapse.

Dr. Willis lived at Greatford, in Lincolnshire, and he there had one of the largest private establishments in the kingdom for the reception of lunatics. His house was pleasantly situated, with grounds and plantations neatly laid out around it, where his patients could enjoy salutary exercise, and have interesting natural objects continually before their eyes. Some of those entrusted to his care were distributed in the neighbouring villages, with proper attendants to wait upon them and overlook their conduct. An asylum in the country is much more adapted "to heal the wounded spirit," and to correct the morbid trains of thought, than a crowded hospital in the midst of a large town. The calm retreat, the multitude of agreeable ob-

jects, the kind and benign aspect of the physician, his constant superintendence, the select society of strangers in similar circumstances, the fear of being put to shame, must all have a favourable effect in promoting the cure. Dr. Willis never published any account of his method; but from what he declared upon a memorable occasion, when the question of a regency was warmly discussed, when he distinguished himself by the manly and bold avowal of his opinion, by giving a favourable prognosis with regard to his majesty's illness, it appears, that of a very great number of persons afflicted with mental derangement, nine out of ten recovered, particularly those who had been placed under his care within three months after they had begun to be afflicted with the disorder. This statement, however, has been declared incorrect, as far as it relates to the precise number of cases, for, in some kinds of insanity, the proportion of cured is greater, in others it is less.

From what can be gathered from the few sources of information to which we have access, the patients at Greatford were treated like human beings; they were managed like children, and not shut up or chained down like wild beasts. Moral means were principally trusted to, yet physical ones were never neglected. The patients were taught to fear and to love their physician; they were admitted to breakfast and dine with him, and in the company of others, as long as they behaved well, and could restrain their feelings; whenever any one transgressed the rules of good behaviour, the strait-waistcoat was employed as the badge of disgrace. No chains or bandages were ever made use of, and the strait-waistcoat only in cases of extreme necessity. One of the principal means he employed was to have the attendants or keepers always appear as servants to the lunatics, appointed to wait upon them and to supply all their wants; but these servants were under the control, and received instructions from their master, the physician, which they dared not, on any account, disobey, and they could not allow any orders to be infringed against without permission. Besides relieving the attendants from a disagreeable situation, this served to give the patients a very high idea of the power of their physician, of whom they never can be brought to think too highly: they ought to feel that their fate rests in his hands,

and that no person in the world is of greater consequence than he who has the management of their health.

Of physical remedies, emetics were most preferred by Dr. Willis. Opium, he thought, did harm; and when narcotics were required, he employed hyosciamus. Blisters on the neck he found hurtful. When the strength of the body wanted to be increased, he gave the Peruvian bark, and he used this in large quantities. Digitalis was considered serviceable in some cases. Dr. Joseph Frank learnt thus much in conversation with Dr. John Willis, and one other fact, which is of some importance, viz. that Dr. Willis had never observed any thing peculiar in the formation of the skull among his patients.—(*Vide Reise nach London, 2ter Band.*)

Various objections are raised against sending a madman to a place of confinement, even among the highest and lowest classes of society. A principal one is the fear of severe and cruel treatment, and the hazard of making the disorder permanent; but these apprehensions are groundless; for there must be some grievous defect in the mode of correcting the disordered mind, if correct sentiments and rational and orderly behaviour are not inculcated by the habit of self-denial, and strong efforts of the will. Let the appeal be fairly made, by visiting a maniac at his own house, or at a large establishment appropriated for the reception of such persons. Patients in bedlam are, in general, not only more orderly, but more rational and tractable than those in a private house, where the continual fluctuations of temper and apprehension among friends, lead alternately to improper indulgence or undue severity. Such treatment will both confirm the morbid associations, and enervate the natural character, just as sullenness and ill-humour are the natural consequences of irregularity and inattention in the early part of life. If great advantages are derived from lunatic hospitals, (imperfect as they have hitherto been, and little as their managers in general understand the art of forming the characters of their patients), how beneficial may they become under the direction of a benevolent physician, who, instead of confining his efforts merely to make the noisy quiet, and the sullen to eat and speak, labours with redoubled attention to rectify the disorders of the several powers of the mind, to ex-

cite its healthy action, and by the fine, yet simple contrivance of making a society or school of their own, exhibits the interesting sight of the melancholic gaining health and spirits, and the maniac restored to take his station again in the extensive society of the world? Such seems to have been the plan of the late Dr. Willis, and the number of those who have recovered under his care, will best testify how well he reduced such a plan to practice.

MEDICAL INTELLIGENCE.

Process for dissolving Gum Elastic in Æther.

[From the Medical and Physical Journal for November, 1811.]

A pound of vitriolic æther is put into a bottle capable of containing about four pounds of any common liquid. On this æther there are poured two pounds of pure water, the bottle is then stopped, held with the mouth downwards, and strongly shaken, in order to mix them. On discontinuing the agitation, the æther soon swims uppermost; the bottle is still held in the same position, and cautiously opened, keeping the thumb on the mouth of it. The water is by this means easily let off, and collected in a vessel below.

This operation is repeated two or three times with new quantities of water, until the sixteen ounces of æther are reduced to about five ounces. It is this purified remainder that is found to be the most perfect solvent of elastic gum, which is thrown into the æther after being cut into small pieces. It begins to swell in a very short time; the æther penetrates it, and appears to act slowly on it at first, but at the end of five or six hours, or later, the liquor is saturated and remains transparent. If there be a surplus of the gum it falls to the bottom, and on being removed from the bottle may be moulded into any form and will preserve its elasticity.

Having now described the manner of dissolving this curious substance, I shall add the most simple method of applying the part completely dissolved to use. A tube may be made

in the following manner: Prepare a small cylinder of pipe-clay of the diameter and length of the intended tube. It is unnecessary to bake it, but sufficient merely to dry it. The æther saturated with the gum is poured into a glass or tin cylinder a little larger than the clay rod, and filled up to the brim with the said liquor. The operator then plunges the clay pipe into the æther, withdraws it suddenly and allows it to remain for an instant in the air, replunges it anew, and repeats the operation in proportion to the intended thickness of the tube, as each immersion leaves a small coating. The tube being thus formed of the elastic gum, is thrown into water in order to dissolve the clay pipe. This the water very soon effects, and the gum is left in the form of a perfect tube.

By a similar process to the above, elastic bottles, &c. may be formed of any size or dimensions.

There have been admitted into the Lying-in Hospital at Paris, (Maison d'Accouchemens) between the 9th of Dec. 1799, and the 31st of May 1809, 17,308 women, who gave birth to 17,499 children; 189 of them have been delivered of twins, and two only of three children. The proportion of twin cases to single births is 1 to 91.

Two thousand of these women were affected afterwards with illness, or some serious accident; 700 died out of the 2000.

Of the 17,499 births, 16,286 were presentations of the *vertex* to the *os uteri.*

No.		Proportions.	
215	were presentations of the feet	1 to	$81\frac{2}{5}$
296	the breech	1 —	$59\frac{1}{9}$
59	the face	1 —	$296\frac{1}{2}$
52	one of the shoulders	1 —	$336\frac{1}{3}$
4	the side of the thorax	1 —	$4374\frac{3}{4}$
4	the hip	1 —	$4374\frac{3}{4}$
4	the left side of the head	1 —	$4374\frac{3}{4}$
4	the knees	1 —	$4374\frac{3}{4}$
4	the head, an arm, and the cord	1 —	$4374\frac{3}{4}$
3	the belly	1 —	5833
3	the back	1 —	5833

3	the loins	1 to 5833
1	the occipital region	1 — 17499
1	the side, with the right hand	1 — 17499
1	the right hand and left foot	1 — 17499
1	the head, and the feet	1 — 17499
2	the head, the hand, and forearm	1 — 8749½
37	the head and umbilical cord	1 — 473

Of this great number of women 230 were delivered by art, the rest were natural births, being in the proportion of 1 to 76½; 161 were delivered by the hand alone, the children being brought by the feet; 49 were delivered by the forceps, either on account of the small dimensions of the pelvis, the falling down of the umbilical cord, or the wrong position of the head, when the woman was exhausted, or her life was in danger by convulsions, &c.; 13 were extracted by the crotchet after perforation of the head, on account of mal-conformation of the pelvis; in these instances the death of the child was first ascertained.

The cesarian operation was performed in two cases, the diameter of the pelvis being only one inch six lines from sacrum to pubis.

In one, the section of the symphisis pubis was performed, the diameter of the pelvis from sacrum to pubis being only two inches and a quarter.

Gastrotomy was performed once, the fœtus being extra uterine; the child weighed 8lb. 2oz.

FRENCH NATIONAL INSTITUTE.

Abstract of the Report of the National Institute at Paris for the year 1810.

Messrs. Gay-Lussac and Thenard have directed their attention to compare the relative powers and energies of different Galvanic piles. They have discovered that the force of the pile is not increased in proportion to the number of plates. To produce a double effect, the number of plates must be increased eight times. In general, it was found that the quantity of gas the piles will produce, is nearly in proportion to the cube root of the number of plates employed.—Amongst the discoveries

to which the Galvanic pile has given rise, there are few more interesting to general chemistry than the transformation of the alkalies into combustible substances of metallic splendour.

This transformation, first discovered by Mr. Davy, was afterwards doubted by Messrs. Gay-Lussac and Thenard. In their former report they were disposed to consider potassium and sodium as combinations of the alkalies with hydrogen, and to class them amongst the compound substances called hydrurets: subsequent experiments have led them to incline to the opinion of Mr. Davy, and to regard potassium and sodium as simple metallic substances.

M. Berthollet has communicated a process for making the muriate of mercury, called mercurius dulcis or calomel, by passing oxygenated muriatic gas through mercury; it combines rapidly with the metal, and forms with it the muriate of mercury; and as this metallic salt has a perfect analogy with other mercurial salts produced by other acids and mercury at the minimum of oxidation, he concludes that the mercury, in forming this combination, has been reduced to an oxide by the oxygen of the acid, and not by that of the water.

M. Guyton has directed his attention to the mode of giving a permanent red to glass, by means of copper, which by accident he first discovered might be done. M. Sage has also taken a part in these experiments, with the intent to colour glass red by means of copper and the phosphate of lime, or with bones; and he has shown crystals of glass, from the bottom of the pots used to melt glass in the bottle manufacture at Seves, which had some resemblance to hexaëdral prisms. It is well known that simple means have been discovered to extract soda from common salt. France formerly imported this article, so necessary to the arts: an inconvenience attended the mode of preparing it, from the quantity of acid gas which escaped, and was highly injurious. Amongst the different means of preventing this inconvenience which have been attempted, that of M. Pelletan the younger is deserving of notice. It consists in making the muriatic gas pass through long horizontal tubes partly filled with calcareous earth, which absorbs the gas, forming with it the muriate of lime. The experiments of M. Sage on plumbago (black lead) show that this substance does not contain any

iron, but consists of a coaly matter mixed with one tenth part of clay. The fossil carbon of St. Symphorien, near Lyons, approaches nearer to this substance than any other known mineral.

M. Deyeux has presented to the class of agriculture a loaf of sugar made from the red beet (*betterave*), which had all the whiteness and flavour of the sugar from the cane. He has announced that this substance may be made in great quantities by the proprietors, who have devoted to this attempt 400 acres of ground. Should it succeed on the great scale, it will change the relations of the two worlds.

Letter from the National Vaccine Board to the Governors of the Finsbury Dispensary, London, on the bad effects of the practice of inoculating for the Small-Pox, at that Charity.

SIR, Leicester Square, Oct. 25, 1810.

As the Finsbury Dispensary was instituted from the purest motives of charity and benevolence, it is conceived, that you and the other governors will approve of a communication, intimating, that the practice of the dispensary in one point is producing effects the reverse of your intention.

It is scarcely requisite to inform you that parliament, after fully examining into the merits of the discovery of vaccination, were convinced of the important benefits that would result from its general adoption; and parliament, in consequence, have authorised Government to form a National Vaccine Establishment, in order to extend vaccination to all parts of the empire.

This establishment is placed under the control of the president and censors, and the master and governors of the royal colleges of physicians and surgeons of London; who, from experience, and authentic information, are fully persuaded of the wisdom of the practice of vaccination, and of the futility of those objections which have been urged against it. They observe, however, with regret, that although vaccination has spread not only through the British empire, but also among foreign nations, with the most beneficial consequences, yet from prejudices artfully kept up, small-pox inoculation is still prevalent among the poor of this metropolis.

About two years ago, the governors of the smallpox hospital took this business into consideration, and from a conviction of the great mortality which was caused by propagating a contagious and fatal disease, they prohibited their medical officers from inoculating for the smallpox, or distributing smallpox matter.

A decrease of deaths from the small-pox was the immediate consequence: but unhappily, this fatal malady again rages through the town, and destroys, within the bills of mortality alone, about thirty persons every week.

Upon investigating the sources of this direful pestilence, I have learnt, that besides the pernicious activity of certain private practitioners, the inoculation of smallpox is extensively carried on at the Finsbury Dispensary; above a thousand persons having been inoculated there last year, and matter having been distributed for the inoculation of others to an unknown extent.

You are earnestly requested to reflect upon the consequences of thus diffusing among the crowded lanes, courts, and alleys of this populous city, a baneful infection, which proves fatal to multitudes, and strikes many with total blindness.

These indisputable facts made an irresistible impression upon the minds of the governors of the small-pox hospital; and as they knew that the inoculation of the small-pox was disapproved of by the most eminent of the medical profession, they were little affected by the misrepresentations of the prejudiced, or by the sophistry of a few who are interested in the propagation of small-pox.

It is to be presumed that when the governors of the Finsbury Dispensary deliberate upon this subject, they will decide with equal prudence and humanity. For they are at present, though no doubt unintentionally, opposing a most benevolent measure of parliament; and sanctioning a practice most fatally injurious to the community. I have the honour to be, &c,

JAMES MOORE, Director.

Extract of a letter from Dr. Seaman, of New York, to one of the Editors, dated 2d mo. 2d, 1812.

"We have had a pretty full opportunity in our hospital of testing the efficacy of the muriate of gold, as a remedy in syphilis, according to the recommendation of Dr. Chrestien; and it really appears to be a valuable discovery. Many cases have been cured by it alone, and apparently as readily and effectually as by mercury, while it produces no disagreeable or dangerous effects upon the system; no evident operation upon the secretions, excepting the increased discharge of urine, and this does not take place uniformly. In a great proportion of the cases it seems to act like a charm, its only evident effect being that of curing the disease."

PHILADELPHIA DISPENSARY.

Managers, elected January 1812.

William White,	Robert Ralston,
Lawrence Seckel,	Ebenezer Hazard,
Robert Blackwell,	Isaac Snowden,
Henry Helmuth,	Joseph Crukshank,
Robert Smith,	Elliston Perot,
Godfrey Haga,	Samuel P. Griffitts.

Officers, elected January 1812.

ATTENDING PHYSICIANS AND SURGEONS.

Doctor Joseph Parrish,
Isaac Cleaver.
John Perkin,
Henry Neill,
Samuel Stewart,
George S. Schott.

CONSULTING PHYSICIANS AND SURGEONS.

Doctor Benjamin Rush,
Thomas Parke,
Caspar Wistar,
Philip S. Physick.

TREASURER.

John Clifford.

APOTHECARY.

George G. Tresse.

HUMANE SOCIETY.

Managers, &c. elected March 1812.

Joseph Crukshank, *President.*
Isaac Snowden, *Secretary.*
William Leedom, } *Inspectors of the Apparatus.*
John Bacon, }
Samuel P. Griffitts, } *Committee of Correspondence.*
Thomas C. James, }
Joseph Parrish, }
Charles Marshall,
Samuel Pancoast, jun.
Benjamin Thaw,
Charles Penrose,
Matthew L. Bevan,
Joseph P. Horner, *Treasurer.*

THREE cases have lately occurred in Guy's Hospital, in which the external iliac artery has been tied with complete success, for the cure of inguinal aneurism. In one case the tumor was in a state of mortification at the time of the operation. The coagulum was discharged, and the patient is daily regaining the use of his limb. This is the fourth time the operation has been performed in that hospital, and all the patients are at this time alive and doing well. In two of them, mortification had taken place upon the tumors at the time of the operation. *Lond. Med. Review, July* 1812.

From a letter from A. Pearson, to the editors of the London Medical and Physical Journal, dated Canton, March 23, 1811, it appears that the practice of vaccination had been carried on extensively to that date, both there and at Macoa, during the ravages of the smallpox, (the visits of which are in that country annual), but when the immediate danger past over it had been so much slighted as to render the preservation of the virus difficult. "Indeed," says the writer, "from August 1810, to "January 1811, we were entirely without it at both places; "and when in consequence of the reappearance of the small-

"pox, applicants for vaccination became numerous, I had the "satisfaction to find that it had been kept up in a district at "some distance from hence *by a Chinese* whom I had in- "structed, and of receiving from that quarter a fresh stock, "from which the practice is now amply kept up here, and has "been revived at Macao." *Lond. Med. and Phys. Journal, December* 1811.

From the tables of the deaths by smallpox in London, in the twelve years preceding the practice of vaccination, and in the twelve years immediately succeeding the introduction of that process, (that is, from 1787 to 1810 inclusive), it appears that,

The total number of deaths by smallpox, in the first twelve years, amounts to - - - -	22,170
Ditto in the second twelve years, or subsequent to the introduction of vaccination, - - - -	17,122
The excess of number of deaths in the first term of twelve years, - - - - - -	5,048

That is, about four hundred and twenty persons per annum fewer for twelve years died since, than before vaccination. *Ibid.*

The operation for the varicose *vena saphena major* has very lately received considerable improvement. The mode of operating formerly employed was too frequently followed by fatal results. The present method, the improvement of which consists in removing the ligature from the vein in a few minutes after its application, we are assured, is always effectual to obliterate the canal of the vessel, and has not yet been succeeded by any untoward circumstance. We are informed that Mr. Freer, of Birmingham, has employed the operation thus modified, several times with complete success. *Ibid.*

Hufeland has lately recommended belladonna in whooping-cough. It has often produced favourable effects in a few days in cases even where opium and musk had been inefficacious. The dose for children, from three to six years, is one fourth of a grain, night and morning; in some cases, the dose may be increased and repeated oftener. *Ibid.*

OBITUARY.

Died, at Kingston, (Jamaica,) T. DANCER, M. D. author of various medical works.

—— at Gloucester, (England,) aftera few days' severe illness, CHARLES BRANDON TRYE, Esq. F. R. S. and senior surgeon to the Glocester infirmary, and author of several interesting works on medicine and surgery.

—— at New York, March 1812, EDWARD MILLER, M. D. in the fifty-second year of his age. He was a native of the state of Delaware, where he practised as a physician; after retiring, at the close of the war, from a situation in the hospital department of the United States, in which his youthful zeal had engaged him. About fifteen years since, he removed to the capital of the state of New York, leaving behind him a character for talents and humanity, which had endeared him to his own state. At New York he continued his professional career with increased ardor and success; not solely confining himself to the practice of his profession, but embarking in literary works for the advancement of medical knowledge, which claim a high rank in science.

With the aid of Dr. Mitchell, he conducted the Medical Repository; a publication whose character is too well known to need a notice of it here.

His rank as a physician had entitled him to the confidence and reward of his adopted state; and he filled, at the period of his decease, the office of resident physician for the port of New York.

FOR THE ECLECTIC REPERTORY.

Statement of Deaths, with the diseases and ages, in the City and Liberties of Philadelphia, from the 1*st of January* 1811, *to the* 1*st of January* 1812.

DISEASES.	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	From 100 to 110	Ages unknown	Total
Abortion - - - -	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2
Angina Pectoris -	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Asthma - - - -	0	0	1	1	0	0	0	1	2	2	0	1	0	0	0	8
Abscess - - - -	1	1	1	1	1	3	2	1	2	1	1	0	0	0	0	15
Aneurism - - - -	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Apoplexy - - - -	0	0	1	1	1	4	10	10	9	4	4	2	0	0	0	46
Atrophy - - - -	3	3	0	0	1	0	0	2	3	2	0	0	0	0	0	14
Burns - - - -	2	1	4	0	0	1	2	1	0	1	0	0	0	0	0	12
Cancer - - - -	0	0	0	0	0	2	0	3	5	0	2	0	0	0	0	12
Caries of the Spine	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0	1
Casualties - - - -	2	1	0	3	3	1	4	1	1	0	0	0	0	0	0	16
Catarrh - - - -	5	1	0	0	0	2	1	0	0	1	0	0	0	0	0	10
Child Bed - - -	0	0	0	0	0	2	1	2	0	0	0	0	0	0	0	5
Cholera Morbus -	147	51	24	1	1	3	3	1	0	0	0	0	0	0	9	240
Cholic - - - - -	0	0	0	0	0	0	3	3	3	0	2	0	0	0	0	11
Consumption of Lungs	9	6	4	8	20	99	74	54	32	16	12	0	0	0	35	369
Convulsions - - -	104	12	13	5	0	7	3	0	3	0	0	0	0	0	15	162
Chicken Pox - -	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Decay - - - - -	11	4	4	2	2	8	5	8	6	8	5	2	0	0	21	86
Diarrhœa - - - -	2	1	1	1	0	6	8	4	1	7	4	0	0	0	0	35
Dropsy - - - -	3	1	5	1	3	11	13	13	11	8	5	1	0	0	0	75
of the Breast - -	2	1	0	2	4	1	4	5	12	2	2	0	0	0	0	35
in the Brain - -	14	12	6	2	1	2	0	0	0	0	0	0	0	0	10	47
Drowned - - - -	0	0	0	2	4	2	17	6	1	0	0	1	0	0	0	33
Dysentery - - -	2	7	7	1	1	3	2	5	8	5	6	0	1	0	0	48
Drunkenness - -	0	0	0	0	0	3	3	0	1	1	0	0	0	0	0	8
Diseases in knee joint	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	1
Debility - - - -	11	1	3	1	1	3	5	2	7	5	8	1	0	0	15	63
Epilepsy - - - -	0	0	0	1	3	4	2	0	1	0	0	0	0	0	0	11
Eruptions - - - -	0	0	0	0	0	1	0	0	0	0	0	0	0	0	0	1
Erysipelas - - -	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	3
Fracture - - - -	0	0	0	0	0	1	1	0	0	0	0	0	0	0	0	2
Fever - - - - -	8	3	3	1	1	9	3	0	2	1	0	1	0	0	0	32
Intermittent - -	2	0	0	0	0	1	1	1	0	0	0	0	0	0	0	5
Remittent - - -	0	1	2	1	3	4	1	0	1	1	0	0	0	0	0	14
Bilious - - - -	0	1	2	4	1	6	1	7	0	1	0	0	0	0	0	23
Nervous - - -	1	0	2	0	2	2	0	2	1	0	0	1	0	0	0	11
Malignant - - -	0	0	1	0	2	1	1	0	0	0	0	0	0	0	0	5
Typhus - - -	0	0	2	3	7	7	10	9	4	1	0	0	0	0	0	43
Puerperal - - -	0	0	0	0	0	4	0	0	0	0	0	0	0	0	0	4
Hectic - - - -	0	0	1	1	0	0	3	0	1	0	0	0	0	0	0	6
Scarlet - - - -	0	0	2	0	0	0	0	1	0	0	0	0	0	0	0	3
Inflammatory - -	1	1	0	0	0	1	1	0	0	0	0	0	0	0	0	4
Mortification and Gangrene	5	2	2	0	2	3	4	3	1	1	0	0	0	0	0	23
Gout - - - - -	0	0	0	0	0	0	1	0	0	3	0	0	0	0	0	4
Gravel - - - - -	0	0	0	1	0	0	0	0	0	1	0	0	0	0	0	2
Carried over,	341	114	91	44	65	208	191	145	118	72	51	10	1	0	105	1553

DISEASES.	Under 1 year	From 1 to 2	From 2 to 5	From 5 to 10	From 10 to 20	From 20 to 30	From 30 to 40	From 40 to 50	From 50 to 60	From 60 to 70	From 70 to 80	From 80 to 90	From 90 to 100	From 100 to 110	Ages unknown	Total
Brought forward,	341	114	91	44	65	208	191	145	118	72	51	10	1	0	115	1558
Hooping Cough - -	15	19	16	2	1	1	0	0	0	0	0	0	0	0	0	54
Hives - - - - - -	28	3	8	1	0	0	0	0	0	0	0	0	0	0	0	40
Hernia - - - - - -	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	1
Hemorrhage - - -	1	1	0	0	2	3	1	0	0	0	0	0	0	0	0	8
Hydrophobia - - -	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
Inflammation of the Brain	2	0	1	1	0	3	5	1	1	0	0	0	0	0	0	14
of the Lungs - -	4	0	0	2	1	1	1	0	1	0	0	0	0	0	0	10
of the Stomach -	1	1	2	0	3	3	2	5	1	0	0	0	0	0	0	18
of the Bowels - -	3	2	4	4	2	5	2	0	2	3	0	0	0	0	0	27
of the Liver - -	1	1	0	2	0	4	5	9	2	2	0	0	0	0	0	26
Insanity - - - -	0	0	0	0	0	5	14	11	1	1	0	0	0	0	0	32
Jaundice - - - -	0	0	0	0	0	1	1	0	1	0	0	0	0	0	0	3
Locked Jaw - - -	0	0	0	1	2	2	2	1	0	0	0	0	0	0	0	8
Measles - - - -	0	0	2	0	0	0	0	0	0	0	0	0	0	0	0	2
Old Age - - - -	0	0	0	0	0	0	0	0	0	1	13	16	10	4	0	44
Pleurisy - - - -	6	4	2	2	9	12	12	5	8	5	2	0	0	0	0	67
Palsy - - - - -	0	0	0	0	0	0	3	3	8	3	10	1	0	0	0	28
Rheumatism - - -	0	1	0	0	0	1	4	1	0	2	0	0	0	0	0	9
Rupture - - - -	0	0	1	0	1	1	0	0	0	1	0	0	0	0	0	4
Scrofula - - - -	2	4	2	0	1	3	2	1	0	0	1	0	0	0	0	16
Sore Throat - - -	11	4	4	1	0	0	1	1	0	0	0	0	0	0	0	22
Small Pox natural -	18	20	21	11	10	13	6	0	3	0	1	0	0	0	10	113
Inoculated - - -	0	3	0	1	0	0	0	0	0	0	0	0	0	0	0	4
Still Born - - - -	137	0	0	0	0	0	0	0	0	0	0	0	0	0	0	137
Suicide - - - -	0	0	0	0	0	1	0	0	1	0	0	0	0	0	0	2
Sudden - - - -	0	1	0	0	1	11	29	11	2	0	0	0	0	0	0	55
Syphilis - - - -	2	0	0	0	1	2	1	0	0	0	0	0	0	0	0	6
Teething - - - -	8	9	3	0	0	0	0	0	0	0	0	0	0	0	0	20
Thrush - - - -	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1
Ulcers - - - - -	2	0	1	1	1	1	0	2	1	0	1	0	0	0	0	10
Worms - - - -	0	3	6	3	0	0	0	0	0	0	0	0	0	0	0	12
Unknown - - - -	5	1	0	1	1	0	8	11	8	4	0	0	0	0	0	39
Total,	588	188	164	77	101	281	290	208	158	95	79	27	11	4	115	2386

NOTE. Of the above there were 719 males of twenty years and upwards, 562 under twenty years: of females 525, of twenty years and upwards, 433 under twenty years; and 147 children, principally under one year, whose sex is unknown.

Deaths in each month of the above period.

	Adults.	Children.
January - - - -	79	69
February - - - -	79	78
March - - - - -	102	87
April - - - - -	95	60
May - - - - - -	105	74
June - - - - -	97	80
July - - - - -	125	126
August - - - -	124	203
September - - -	104	92
October - - - -	131	107
November - - -	100	95
December - - -	100	74
Total	1241	1145

By order of the Board of Health,
JOHN ALLISON, Clerk.

Health Office, February 24th, 1812.

It is supposed that the population of the city and suburbs of Philadelphia (within the range of the foregoing Bills of Mortality) founded upon the enumeration of the last census, amounts to about ninety-five thousand.

RECENT FOREIGN PUBLICATIONS.

Practice of Medicine.

A Collection of Treatises on the effects of Sol-lunar influence in Fevers, with an improved method of curing them. By Francis Balfour, M. D. 8vo. Second edition.

An Essay on the disease called Yellow Fever, with observations concerning febrile contagion, typhus fever, dysentery, and the plague, &c. By Edward Nathaniel Bancroft, M. D. &c. &c.

Popular Directions for the treatment of the Diseases of Women and Children. By John Burns. London and New York republished.

A Popular Treatise on the causes and distinction of the varieties of the Asthma, with new and simple and successful instructions for the prevention and treatment of Asthmatic Fits, &c. &c. By T. M. Caton, M. D.

Illustrations of Madness, exhibiting a singular case of insanity, and a no less remarkable difference in medical opinion. 8vo. By John Haslam. London.

Hints for the construction, economy and moral management of an Hospital for the Insane; pointing out the existing defects in such institutions, and proposing means for ameliorating the condition of maniacs. By John Haslam.

A Paper containing the results of eleven years practice at the original Vaccine Pock Institution, No. 44, Broad street, Golden square, London.

Essays on the changes of the Human Body at different ages, the diseases to which it is predisposed in each period of life, and the physiological principles of its longevity. By Thomas Jameson, M. D. 8vo.

A Treatise on Gout, containing the opinions of the most celebrated ancient and modern physicians on that disease,

and observations on the Eau Medicinale. By John King. 8vo.

History of the Walcheren Remittent, &c. &c. By Thomas Wright, M. D.

A Posologic Companion to the London Pharmacopœia. By John Nott, M. D. 18mo.

Cursory remarks on Contagious Diseases, and on Baths. By M. L. Este, esq. 8vo.

A Letter to the Physicians and Surgeons of St. George's Hospital, on Mr. Davy's simple Galvanic Circles, considered as a topical assistant branch of medicine, &c. By Matthew Yatman. 8vo.

Natural History of the Human Teeth, with a treatise on their diseases, &c. By Joseph Murphy. 8vo.

Nouvelles Observations recueillies sur l'Elephantiasis des Arabes, &c. Par M. Alard. Paris, 1811.

Traite d'Hygiene, applique à la Therapeutique. Par J. B. G. Barbier. 8vo. Paris, 1811.

An Inquiry into the nature, causes and cure of Hydrothorax; illustrated by interesting cases, &c. By L. Maclean, M. D.

The new London Practice of Physic, &c. &c. with prescriptions according to the nomenclature of the New Pharmacopœia. Seventh edition, enlarged and carefully revised, by E. G. Clarke, M. D. 8vo.

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Observations on the use of Caustic Alkali in scrophula and other chronic diseases. By Joseph Brandish. 8vo.

Anatomy, Surgery, and Midwifery.

A new description of the Muscles of the Human Body, accompanied by about 15 engravings of the principal muscles. By John James Watt, surgeon.

Essays on the Anatomy of Expression. By Charles Bell. 4to.

Anatomy of the Human Body; by John and Charles Bell. 3d edition, in 3 vols. 8vo.

The Morbid Anatomy of the Gullet, the Stomach and Intestines. By Alexander Munro, jun. M.D. F.R.S.E. Professor of Anatomy and Surgery in the university of Edinburgh.

Practical Observations on various novel modes of operating on Cataract, and of forming an artificial pupil. By Robert Muter Holbeach, member of the Royal College of Surgeons. London. 8vo.

Observations on the Cataract and Gutta Serena; including a translation of Wenzel's treatise on the Cataract, a new chapter on the operation of largely puncturing the capsula of the crystaline humour, and many additional remarks on the gutta serena. By James Ware, F.R.S.

Practical Observations on the treatment of the diseases of the Prostate Gland, illustrated by copperplates. By Everard Home, esq. F.R.S.

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Practical Observations on the Sclerocele, and other morbid enlargements of the Testicle; also on the cause and cure of the acute, the spurious, and the chronic Hydrocele. The whole illustrated by cases, &c. By Thomas Ramsden, surgeon, &c. &c. 8vo.

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Nouvelles remarques sur les Hernies Abdominales. Par M. Lordat, chef de travaux anatomiques de la Faculté de Medicine de Montpellier. Montpellier, 1811.

Chemistry, Natural History, Botany, &c.

Hortus Cantabrigiensis; or a catalogue of Plants, indigenous and exotic. By James Donn, curator. Sixth edition. 12mo.

Outlines of a course of Natural Philosophy; part 1st. By John Playfair, F.R.S. London and Edinburgh, professor of natural philosophy in the university of Edinburgh. One volume, 8vo.

Recherches Physico-chimiques, faites sur la Pile, sur la preparation chimiques et les proprietés du Potassium et du Sodium, &c. &c. Par MM. Gay-Lusac et Thenard, membres de l'Institut. Two vols. 8vo. Paris, 1811.

Traité de Pharmacie, theorique et pratique, contenant les elemens de l'histoire naturelle de tous les medicamens, leur preparations, &c. &c. Par J. J. Verey. Two vols. 8vo. Paris, 1811.

Memoirs of the Wernerian Natural History Society. Vol. 1st, for the years 1808-9-10, with fifteen engravings. 8vo.

Elements of Chemistry. By J. Murray, lecturer on chemistry, &c. Edinburgh. Two vols. 8vo.

The Organic Remains of the former World, containing the remains of insects, &c. &c. with coloured plates. Third and concluding volume. By James Parkinson.

RECENT AMERICAN PUBLICATIONS.

The Edinburgh Encyclopædia. By David Brewster, L. L. D. F. R. S. E. Republished with considerable improvements and additions. By Edward Parker and Joseph Delaplaine, booksellers, Philadelphia. The first part of the 1st volume is published; the second part is in press, and will be published next month.

The New England Journal of Medicine and Surgery and the Collateral Branches of Science; conducted by a number of Physicians. No. 1. vol. I. published quarterly, by Thomas B. Wait & Co. Boston.

A Treatise on the Process employed by Nature, in suppressing the Hemorrhage from divided and punctured Arteries, and on the use of the Ligature; concluding with Observations on Secondary Hemorrhage, &c. &c. By J. F. D. Jones, M. D. member of the Royal College of Surgeons, London. T. Dobson, Philadelphia, illustrated by 15 plates.

Surgical Observations on Injuries of the Head, and on Miscellaneous Subjects. By John Abernethy F. R. S. Philadelphia, Thomas Dobson.

A Treatise on a Malignant Epidemic, commonly called Spotted Fever; interspersed with Remarks on the Nature of Fevers in general. With an appendix. By Elisha North. 12mo. New York. T. & J. Swords.

An Inaugural Dissertation on the Use of the Digitalis Purpurea, in the cure of certain Diseases. By Thomas Edward Steell, of New Jersey, 8vo. New York. T. & J. Swords.

A System of Anatomy for the Use of Students of Medicine. By Caspar Wistar, M. D. Professor of Anatomy in the university of Pennsylvania, vol. I. pp. 422. Philadelphia, Thomas Dobson.

A Treatise on Ruptures, containing an anatomical description of each species, with an account of its symptoms, progress and treatment. By William Lawrence, member of the royal college of surgeons. With an appendix. By Joseph Parrish, M. D. one of the surgeons of the Philadelphia almshouse. pp. 412. Philadelphia, Edward Parker.

PROPOSED AMERICAN PUBLICATIONS.

Thomas Dobson has in Press,

The History of North Carolina. By Hugh Williamson, M. D. L. L. D.

Also, preparing for Publication,

Cavallo's Elements of Natural Philosophy. Condensed and improved by the Rev. Mr. Brosius.

A. Finley has in press,

Corvisart on the organic diseases and lesions of the Heart and large Vessels. 8vo. It will be published about the 1st of May.

Charles Bell's Engravings of the Arteries, illustrating the anatomy of the human body, and serving as an introduction to the surgery of the arteries; embellished with fourteen fine engravings. Subscription price five dollars.

The Editors of the Eclectic Repertory have observed with satisfaction, that selected matter from the most approved European publications, has been timely furnished them to carry on the work hitherto. And as, by late arrangements, they expect to be regularly supplied with the most interesting Medical Journals and Reviews of Great Britain and the Continent of Europe, they can safely promise a continuation of interesting matter, and regularity of publication.

But as part of their plan embraces original papers, they take the present occasion of soliciting such assistance in this way as will enhance the value of the work; and particularly request any notices of the climate and diseases of different parts of our country, and of the many medicinal waters which abound in the United States.

Communications of the above nature may be forwarded to Thomas Dobson, the publisher, No. 41, South Second street, Philadelphia.

THE

ECLECTIC REPERTORY

AND

ANALYTICAL REVIEW.

Vol. II. JULY, 1812. No. VIII.

SELECTED PAPERS.

On the non-existence of Sugar in the Blood of persons labouring under Diabetes Mellitus. In a Letter to Alexander Marcet, *M. D. F. R. S. from* William Hyde Wollaston, *M. D. Sec. R. S.*

[From the Philosophical Magazine for July 1811.]

MY DEAR SIR,

IN reply to your inquiry respecting my experiments upon the non-existence of sugar in the serum of diabetic persons, which I have mentioned to you at different periods, I am really ashamed to reflect how long I have suffered them to remain neglected, when I consider their tendency to elucidate a curious point of physiological research.

My first endeavours to detect sugar in the serum of the blood were made soon after perusing the second edition of Dr. Rollo's Treatise on the Diabetes, (which was published in 1798,) at the request of Dr. Baillie, who was so obliging as to furnish me with various specimens of diabetic blood and serum for this purpose.

The other set of experiments which I made with reference to the same question were not thought of till the following year. The inquiry was then left unfinished, and I never resumed it;

for as I soon after* relinquished the practice of physic, I desisted in a great measure from prosecuting any inquiries connected with medicine.

However, since so much of this subject as is strictly physiological, relating to the natural course of circulating fluids, and more especially so much of the investigation as is conducted by chemical means, is within the range of those pursuits which are generally interesting to the Royal Society, I will endeavour to give you as distinct an account as I am able of the progress of my own experiments, requesting that you will in return state, more fully than you have hitherto done, the result of that further step in the inquiry which you took at my suggestion; and if it is agreeable to you, we will without delay make a joint communication of our researches to the Society.

Although Dr. Rollo had been assisted in the chemical part of his inquiry by the well-known talents of Mr. Cruickshank, it appears that they "had not been so fortunate as to obtain a sufficient quantity of serum for chemical experiment†;" and were unable fully to satisfy themselves, by the taste or by other means which they could employ, concerning the existence or non-existence of sugar in the blood of persons labouring under diabetes; but nevertheless they were persuaded of its presence.

For the purpose of forming some judgment on this question, Mr. Cruickshank made trial of the quantities of oxalic acid that could be formed from serum or from blood in their natural state, and from the same serum or blood after the addition of a certain proportion of sugar; and from the difference perceptible in these trials, he formed a probable conjecture respecting the presence or absence of sugar in the serum of diabetic persons.

This method, it is evident, is liable to a twofold objection: first, that an excess of other ingredients beside sugar will cause an increase of the quantity of oxalic acid formed; and secondly, that slight variations in the process for forming oxalic acid will unavoidably occasion differences in the result.

* In 1800. † Rollo on Diabetes, p. 408.

The method which I employed appears to me capable of detecting much smaller quantities of such an ingredient; for though it might not enable us to distinguish exactly the nature of any small quantity that may be discovered, still the mere question of absence or presence admits of determination with great precision.

For this purpose I investigated, in the first place, how the albuminous part of healthy serum could be most completely coagulated, and by what appearances the presence of sugar that had been added to it would be most easily discerned.

When heat alone had been employed for the coagulation of serum, to which water had been added, that which exuded from it was still found to contain a portion of albumen dissolved in it; and if this were allowed to remain, any saccharine matter which might be present would be disguised, and could not with certainty be detected.

I found, however, that this residuum of coagulable matter might be altogether prevented by the addition of a small quantity of dilute acid to the serum before coagulation.* To six drachms of serum I added half a drachm of muriatic acid previously diluted with one drachm and a half of water, and immersed the phial containing them in boiling water during four minutes The coagulation was thus rendered complete. In the course of a few hours a drachm or more of water exudes from serum that has been so coagulated. If a drop of this water be evaporated, the salts which it contains are found to crystallize, so that the form of the crystals may be easily distinguished; they are principally common salt.

If any portion of saccharine matter has been added to the serum previous to coagulation, the crystallization of the salts is impeded, or wholly prevented, according to the quantity of sugar present.

If the quantity added does not exceed two grains and a half to the ounce, the crystallization is not prevented; but even this small quantity is perceptible by a degree of blackness that ap-

* I presumed that this portion of albumen was retained in solution by the alkali redundant in serum, and added the acid for the purpose of neutralizing it.

pears after evaporation; occasioned, as I suppose, by the action of a small excess of acid on the sugar.

If five grains have been added, the crystallization is very imperfect, and soon disappears in a moist air by deliquescence of the sugar. The blackness is also deeper than in the former case.

By addition of ten grains to the ounce, the crystallization of the salts is entirely prevented, and the degree of blackness and disposition to deliquesce are of course more manifest than with smaller quantities.

As I was aware that the sugar obtained from diabetic urine is a different substance from common sugar (approaching more nearly to the sugar of figs), I had the precaution to repeat the same series of experiments upon serum, to which I made corresponding additions of dry sugar, that I had formerly extracted from the urine of a person who voided it in considerable quantity; and I found the effects to be perfectly similar in every respect.

As a further test of the absence or presence of sugar, I found it convenient to add a little nitric acid to the salts that remained after crystallization of the drop. If the serum has been successfully coagulated without any addition of sugar, the addition of nitric acid merely converts the muriatic salts into nitrates, and nitrate of soda is seen to crystallize without foam or blackness. But when sugar has been added, a white foam rises round the margin of the drop; and if further heat be applied, it becomes black in proportion to the quantity of sugar present.

Such are the appearances when the proportions have been duly adjusted, and the proper heat for coagulation applied. I must own, however, that I could not always succeed to my satisfaction at the time when these experiments were conducted, and I am inclined to ascribe occasional failures to having used more muriatic acid than was really necessary, which by excess of heat might redissolve a part of the coagulated albumen, and thence occasion appearances which, without careful discrimination, might be ascribed to sugar.

After having, by this course of experiment, satisfied myself

as to the phenomena exhibited by serum in its natural state, and the effects of any small additions of sugar, I then proceeded to the examination of such specimens of diabetic blood or of serum, as I was able to procure.

The first which I examined was a portion of blood that had been taken from a person whose urine had been analyzed, and found to contain sugar. This blood had been dried, when fresh, by a gentle heat, so as not to coagulate the serum. After being reduced to powder, it was mixed with water, in order that every thing which remained soluble might be extracted. A little muriatic acid was then added, and sufficient heat applied for coagulation of the albumen. The water that separated after coagulation was found to contain the salts of the blood, but no trace whatever of sugar.

A second specimen of dried blood, that had been ascertained to be diabetic on the same evidence as the preceding, was examined in a similar manner, with the same result, as no appearance of sugar could be discerned.

In a third instance, I had some serum from the blood of a person whose urine had been tasted, and found "*very sweet.*" (I had no opportunity of procuring any of this urine for analysis.) After a portion of this serum had been coagulated, with the addition of the usual proportion of muriatic aid, there was no appearance whatever of sugar. But when three grains of diabetic sugar had been added to another ounce of the same serum, the presence of this quantity was manifest by the same process.

I had also a fourth opportunity of examining serum of a person whose urine contained so much saccharine matter, that an ounce of it yielded, by evaporation, thirty-six grains of extract. In this instance I was not so successful in my experiment; for, though I was satisfied that no sugar was present, there certainly was a degree of blackness, which might have been occasioned by about one grain and a half of sugar in the ounce of serum. But this black matter appeared not to be sugar: it was more easily dried than sugar: it was not fusible by heat, as

sugar is: and its refractive power* was too great for that of sugar.

I unfortunately had no opportunity of repeating the experiment on a second portion of the same serum, having inconsiderately employed it for other experiments, and coagulated it at the same time with the former.

In the next experiment I added half a drachm of the urine of the same person to six drachms of the serum, and with a due proportion of diluted muriatic acid coagulated as before. Although the quantity of extract added did not exceed $\frac{36}{16}$, or two grains and a quarter of extract, the difference was very manifest by the darkness of the colour and the defective crystallization of the salts.

To the remaining quantity of the serum I had added twice the former proportion of the urine, and found that this quantity did not wholly prevent the crystallization of the salts during the evaporation of the drop.

The result of these trials was such as to satisfy me that the serum in this instance contained no perceptible quantity of sugar, or at least that the water separable from the coagulated serum did not contain one-thirtieth part* of that proportion which I had found in the urine of the same person.

In order to account for the presence of sugar in the urine, we must consequently either suppose a power in the kidneys of forming this new product by secretion, which does not seem to accord with the proper office of that organ; or, if we suppose the sugar to be formed in the stomach by a process of imperfect assimilation, we must then admit the existence of some channel of conveyance from the stomach to the bladder, without passing through the general system of blood-vessels. That some such channel does exist, Dr. Darwin* endeavoured to ascertain, by giving large doses of nitre, which he could perceive to pass with the urine, but could not detect in its passage through the blood; and he imagined the channel by which it

* The method by which this was tried has since that time been described in the Philosophical transactions for 1802.

* Account of the retrograde Motion of the absorbent Vessels, by Charles Darwin.

was conveyed to be the absorbent system, upon the supposition that they might admit of a retrograde motion of their contents.

Without adopting the theory of Dr. Darwin, it did appear to me that the fact deserved to be ascertained by some test more decisive than nitre; and I conceived that if prussiate of potash could be taken with safety, its presence would be discerned by means of a solution of iron in as small proportion as almost any known chemical test. Upon trial of this salt, I found that a solution of it might be taken without the least inconvenience, and that in less than one hour and a half the urine became perceptibly impregnated, and continued so to the fifth or sixth hour, although the quantity taken had not amounted to more than three grains of the salt.

After a few previous trials of the period when the principal impregnation of the urine might be expected, and when the presence of the prussiate (if it existed in the blood) might with most reason be presumed to occur, a healthy person about thirty-four years of age was induced to take a dose corresponding to three grains and a half of the dry salt, and to repeat it every hour to the third time. The urine being examined every half hour, was found in two hours to be tinged, and to afford a deep blue at the end of four hours. Blood was then taken from the arm, and the coagulum, after it had formed, was allowed to contract, so that the serum might be fully separated. The presence of the prussiate was then endeavoured to be discovered by means of a solution of iron, but without effect; and as I thought that the redundant alkali (which had been ascertained to prevail in this serum) might tend to prevent the appearance of the precipitate, I added a small quantity of dilute acid; but still I could not discern that any degree of blueness was occasioned by it.

This experiment having been repeated a second time with the same result, seemed to me nearly conclusive with respect to the existence of some passage, by which substances certainly known to be in the stomach may find their way to the bladder without being mixed with the general mass of circulating fluids.

Being desirous of ascertaining whether the prussiate could be discovered in any other secretions, I have repeatedly exa-

mined my saliva, at times when the urine has manifested a very strong blue, by adding solution of iron, but I could at no time perceive the saliva to be tinged.

I have also, during a severe cold, accompanied with profuse running of water from the nose, made a similar examination of this discharge, but have not been able to perceive any trace of the prussic acid.

It was nearly in this state that I left the inquiry at the period I have mentioned, and I do not remember to have made any other experiments, when I requested your assistance in making trial of the serum that is secreted in consequence of the application of a blister. Your report upon the result of your experiments, in addition to those which I have above related, nearly satisfied me as to the existence of some unknown channel of conveyance by which substances may reach the bladder.

With respect to Dr. Darwin's conception of a retrograde action of the absorbents, it is so strongly opposed by the known structure of that system of vessels, that I believe few persons will admit it to be in any degree probable.

Since we have become acquainted with the surprising chemical effects of the lowest states of electricity, I have been inclined to hope that we might from that source derive some explanation of such phenomena. But though I have referred* secretion in general to the agency of the electric power with which the nerves appear to be indued, and am thereby reconciled to the secretion of acid urine, from blood that is known to be alkaline, which before that time seemed highly paradoxical; and although the transfer of the prussiate of potash, of sugar, or of other substances may equally be effected by the same power as acting cause, still the channel through which they are conveyed remains to be discovered by direct experiment.

I have, indeed, conjectured that, by examining the blood in the abdominal vessels, or contents of the lacteals, it might be possible to detect them *in transitu;* but I have not been inclined to make such experiments on living animals, as would perhaps throw light upon the subject. I remain, dear sir, with great regard, your's very truly,

W. H. Wollaston.

* Phil. Mag. for June 1809.

History of fatal Effects from the accidental Use of White Lead; in a Letter to the President. By JOHN DEERING, *Surgeon, F. M. S.; with additional remarks by* WILLIAM SHEARMAN, *M. D. F. M. S.**

[From the Philosophical Magazine for August 1811.]

AT the sitting of January 30, 1809, a verbal communication was made to the Society, by the author of the following memoir, of some extraordinary symptoms, followed by the death of several individuals of a family whom he had attended. It appearing to the members present highly probable that these unfortunate events originated from the poison of lead†, a committee was deputed to investigate and to endeavour to detect the real cause of the fatality; which the following relation fully and satisfactorily explains.

Aldersgate Street, Oct. 4, 1809.

If the following narrative do not convey any important medical information, it may not be wholly uninstructive, as it relates to a domestic calamity, occasioned by a circumstance which at the time was wholly unsuspected; and it may at least inculcate the necessity of a closer investigation of symptoms from causes not fully ascertained, and at the same time evince the fallacy of hasty prognostics.

On the 21st of October last, I was desired to visit Mrs. R., the wife of a respectable tradesman in Aldersgate-street, who complained of violent pain in the scrobiculus cordis, with great soreness of the epigastric region when pressed upon. She had vomited a considerable quantity of bilious matter, and at the same time her bowels were constipated: the pulse was calm and regular, the tongue clean and moist, and there was no symptom of fever present. She immediately took a cathartic,

* From the Transactions of the Medical Society of London, vol. i. part i.

† On the following evening Dr. Shearman delivered the annexed communication, which served to confirm the probability of these suspicions; although Mr. D. had been hitherto unsuccessful in detecting the precise origin of the exciting cause.

which operated, and an opiate in the evening. The following morning the patient appeared relieved; in the evening, however, the pains and vomiting recurred, and these symptoms continued for some successive days, in so distressing a degree, that it was deemed advisable to consult the family physician, which was done on November 4, 1808. At this time these symptoms continued as already intimated, without any appearance of fever, and hence the physician was induced to consider the affections as of a rheumatic and spasmodic nature.

In a few days, in consequence of the amendment of the patient, he discontinued his visits. In about a week after this period, a boy in the same family, nearly sixteen years of age, was seized with symptoms exactly similar to those of the preceding case, and similar remedies afforded only partial relief, till at length he was removed into the country, and thereby recovered his health.

A week after the attack of this youth, the eldest child, a boy six years old, was also seized with analogous symptoms, and, the mother having relapsed into her former state, the physician was again consulted on the 19th of November. At this time three other persons in the family laboured under similar affections, and suspicions were now entertained that some poisonous substance might have caused this general indisposition of the family; but after minute investigation, no one circumstance was discovered to confirm this suspicion, or to elucidate the source of so extensive a calamity.

The sickness and pain continued unabated in Mrs. R.; but the son, after the period of a fortnight, was deemed in a state of convalescence by his physician, who discontinued his attendance; he was, however, soon after seized with convulsions, and expired within a few hours. Unexpected and severe as this shock was, Mrs. R. afterwards gradually grew a little better. She had hitherto continued to suckle her child, which, it being fifteen months old, she was advised to wean: to this she reluctantly consented. In about ten days afterwards the child became somewhat costive, without any other apparent indisposition; but at this period it was seized with vomiting and convulsions, and suddenly expired. The unhappy parent now experienced a return of her complaints, and, under a persuasion of the

inefficacy of professional aid, she was prevailed upon to consult an empiric, whose attendance, though continued to the end of the year, proved unavailing; and on the 3d of January, 1809, she had the advice of Mr. Chevalier, an experienced surgeon, who considered the patient's complaint to be chronic rheumatism; and by the use of clysters of warm water, oily mucilaginous medicines, fomentations, and vesicatories, she appeared to experience more relief than at any period since the first attack; but, although the vomiting and sickness were less violent and frequent, the pain and soreness of the abdomen, first complained of, never entirely subsided: she was, however, able to sit up and amuse herself with a little needlework, and even to go about the domestic concerns of the family, and Mr. Chevalier had proposed to pay his final visit on the 21st. On the morning of this day she rose at ten o'clock, and within the space of an hour afterwards, whilst standing near the desk of drawers, she suddenly exclaimed, "I am dying!" She was seized with convulsions, which continued till five o'clock in the afternoon, when she expired.

On the subsequent day, Mr. Chevalier, whose anatomical skill is well known, examined the body by dissection. Neither the thoracic and abdominal viscera, nor the brain, upon the most minute examination, exhibited the least appearance of disease; in short, not the least trace could be discovered of any morbid affection.

With respect to the three other persons already mentioned to have been indisposed, the servant maid, one of them, was conveyed to her friends, and recovered. A sister-in-law of Mrs. R. also recovered; but the third, who was her mother-in-law, died, after lingering under disease till March.

These circumstances having been cursorily communicated to the Medical Society, Dr. Adams, Dr. Hamilton, and Mr. Lawrence, were requested to visit the house of this unfortunate family, and to endeavour to ascertain the cause of the calamity. Every culinary article and the whole premises were accurately examined, but without its leading to any discovery. It appeared, indeed, that Mr. R., the husband of the deceased lady, had purchased a cask of sugar at a sale, a considerable part of which had been disposed of to some friends in the coun-

try, who had used it without inconvenience, and hence no suspicion was entertained of this article having produced the fatality in Mr. R.'s family.

In this state of uncertainty, Dr. Laird, another member of the Medical Society, visited the house; and, on examining the cask which had contained the sugar, he observed a white powder adhering to its inner surface, and which, on being heated by the blow-pipe on charcoal, afforded globules of lead in the metallic state.

The mystery was thus at length developed. The sugar had been injudiciously put into a cask which had previously contained white lead. That part of the sugar which was sent into the country had probably been taken out of the middle of the cask, and had never come in contact which the lead; whilst that which was used by the family, having been taken from the side, was impregnated with this metal, and doubtless was the source of the fatal events described.

Of nine persons in this family, who were more or less indisposed, four died, and the effects of the poison appear to have been nearly in the ratio of their respective ages.

The infant, fifteen months old, was attacked and expired within the space of twenty-four hours; the child six years of age survived a fortnight; Mrs. R., aged forty, lingered three months before the fatal event took place; and the mother-in-law, aged sixty-seven, died four months after the attack.

The symptoms in each were very similar. The vomiting, pain in the stomach, and costiveness, marked the attack of the disease; and the soreness of the epigastric region in those who recovered was not removed by medicine, but seemed rather gradually to wear away by time or change of air. The matter vomited was usually of a dark yellow colour, though sometimes green; the fæces were in general dark-coloured; but in the case of Mrs. R. they were completely white during the space of twenty-four hours only.

There was a considerable sameness in the medical treatment. The opiates which were given afforded no mitigation of the symptoms, unless joined with cathartics, and aided by fomentations, &c. The countenances of all the patients exhibited a

pale, sickly, wan aspect. The pulse in each was slow and regular, rather indeed sluggish, and generally below the natural state; but in no instance was there any symptom of paralysis.

J. DEERING.

Experiments upon Cantharides.

By M. ROBIQUET, Apothecary.

[From the New Medical and Physical Journal, November, 1811.*]

AMONG the number of experiments made upon cantharides, those of Drs. Thouvenel and Beaupoil are principally distinguished; but after bestowing upon each of these physicians all the merit due to them, for having published some interesting results on the chemical nature and remarkable properties of these insects, it must be allowed we are far from possessing an accurate analysis of them, and consequently that it is impossible, at present, to explain in a satisfactory manner, the various effects produced on the animal economy by cantharides.

Fully persuaded of the advantages which would accrue from a more minute investigation of their chemical analysis, I undertook a series of experiments, the results of which I shall presently relate; but I wish previously to state what our present knowledge upon the subject is, as derived from the above-mentioned works of M. M. Thouvenel and Beaupoil. The former gives as the products of his analysis,

1. Parenchymatous matter, forming one half the weight of the cantharides.

2. A black matter, affording by distillation an acid phlegm, and a concrete volatile salt.

3. A greenish greasy matter of an acrid taste, to which he attributes the whole of the odour and causticity of the cantharides.

4. Lastly, A yellow substance, which he regards as wax, not fully elaborated, meriting no consideration in a medical point of view.

M. Beaupoil, in the recapitulation of his chemical analysis, says, that cantharides contain,

* (Annales de Chimie, 1811.)

1. A black extractive matter soluble in water.

2. A yellow matter, equally soluble in water, and separable from the former by alcohol.

3. An unknown acid, but suspected to be the phosphoric.

4. Lastly, A parenchymatous substance insoluble in these fluids, and composed for the most part of an animal matter and phosphate of lime; sulphate, muriate, and carbonate of lime and iron being in very small quantities.

The same author is induced to believe from his physiological experiments, that cantharides contain two principles possessing similar properties; one the green matter, whose action is limited to simply vesicating the skin when applied to it, producing no effect upon the general animal economy; the other the extractive matter, which has the double property of vesicating the skin when applied to it, and is also highly deleterious when introduced into the digestive or circulatory system. This extractive matter is said to be composed of two substances, the one yellow and the other black, to both of which M. Beaupoil attributes a powerful vesicating quality.

It is very difficult to suppose that three different substances should each at the same time possess a quality which is so very rare; and I suspected that this effect, which is so remarkable in cantharides, must be owing to one and the same matter only; upon this supposition, I made a great number of trials, the most conclusive of which I shall detail.

I boiled some cantharides slightly bruised in distilled water; the decoction was of a brownish red colour; it reddened turnsole, and possessed also some other properties which I shall pass over in silence, as being irrelevant to the present subject of inquiry, whose sole object is to investigate the nature of the vesicating matter. I shall, therefore, content myself with saying the decoction possessed this property in a very high degree. Finding that water took up this vesicating substance, whatever it was, I continued boiling the same cantharides until the water was no longer charged with any principle; I then dried the residue and treated it with alcohol, and the green tincture produced from it was freely exposed to the air, to obtain by slow evaporation whatever was contained in solution; by these means I collected a green fluid oil which was not in

the least vesicating. I applied some of it to my lips without their being the least affected by it. Convinced by this, that we had erroneously regarded this green oil as one of the vesicating principles, I examined the aqueous decoction, in order to discover the source of its vesicating property.

I reduced the decoction to the consistence of a soft extract; and in imitation of M. Beaupoil, I separated this extract by means of alcohol, into two distinct and separate parts, one black and insoluble, the other yellow, viscid, and very soluble. Perceiving that the portion soluble in alcohol was strongly vesicating, I was not at all afraid of taking up the black matter, even by the repeated action of boiling alcohol, till it was no longer coloured, notwithstanding M. Beaupoil's advice. I took care to stir up well, with a glass rod, the residue at the bottom of the hot alcohol, so that the whole surface might be successfully presented to it. Having done this, I treated the black matter with water, and was satisfied that it had not preserved the least vesicating quality. Here then, were two essential points determined.

Having thus succeeded in tracing the vesicating principle to that portion of the watery extract soluble in alcohol, nothing more was wanting than to know if the whole of this matter, or a part only, was vesicating. It was a long time before I could come to any conclusion on this subject; I did not fail, however, to submit this substance to a variety of trials, but all the products which I obtained from it were equally vesicating. At last, I treated this yellow matter with rectified sulphuric ether, which at first appeared to have no action upon it; but after having introduced some of the mixture into a vessel hermetically sealed, and agitated it almost continually for several hours, this substance, which in the first instance remained fixed to the sides of the vessel, detached itself by degrees, and was not long in becoming soft and separating. The ether took a slight yellow tint; I decanted it into a porcelain saucer, and as the ether evaporated, small micaceous plates, stained by little drops of a yellow fluid, were deposited. When the evaporation was completed, I treated the residue with cold alcohol, which took up the yellow matter without sensibly attacking the little crystalline plates; these were col-

lected upon bibulous paper and dried; they were insoluble in water, soluble in boiling alcohol, from which they separated upon its cooling, always taking a crystalline form. I employed the above method to obtain this matter in a state of purity, and when I judged it to be so, my first care was to satisfy myself if it was vesicating. For this purpose, I fixed about the hundredth part of a grain at the extremity of a slip of paper, which I applied to the edge of my under lip; in about a quarter of an hour, I began to feel a slight pain in passing my finger over the place; soon after some little blisters formed. Once certain of what I was inquiring after, I put a small quantity of cerate upon the part, to arrest, or at least diminish the effects of the vesicating substance; but it happened that the cerate, by diluting the small quantity I employed, extended its effects over a very considerable surface, and I had both lips covered through their whole extent with bladders filled with serum. I took a very small quantity of this substance and diluted it with two or three drops of oil of sweet almonds; the solution being complete, I covered a small square of varnished paper with this oil and applied it upon my arm; in about six hours a bladder was formed, the whole dimensions of the paper, leaving no doubt that this matter was essentially epispastic. When on the other hand, I examined in a similar way, the yellow matter from which the ether had extracted the vesicating principle, (for so in future I shall call it) I could not discover in it the least trace of this property.

Nothing is more easy than to conceive how this result, notwithstanding its simplicity, had escaped M. Beaupoil. The cause must necessarily be in the manner of operating; indeed, this physician had only infused the cantharides in hot water; but as the vesicating principle is not soluble by itself in water, as I have already shown, it could only be continued in it by means of some other substance, such as the yellow matter; it could, therefore, only be totally taken up by this vehicle by increasing its power to the utmost, especially if we consider that the green oil remaining in the cantharides after the watery infusion must retain, as an oily body, a great affinity for the principle in question, since it is soluble in oils. Besides, M. Beaupoil directs that the watery extract should be treated by

alcohol in the cold; for, says he, if the slightest degree of heat be employed, the substances which have been separated will again unite. For my part, I have not observed this to be the case; it is true, that before putting the extract into alcohol, I dried it as much as possible without injuring it, and employed highly rectified alcohol; I also used a large quantity, otherwise the humidity which remains in the extract, dilutes the alcohol, and favours the solution of the black matter, especially if heat be employed; but in following the mode I have pointed out, the black matter is not attacked even by the boiling alcohol; on the contrary, it becomes more dry and friable in proportion as the yellow matter is taken up, even so as to be no longer softened by the heat.

I think I have sufficiently shown, not only that the green oil and the black matter are not at all vesicating, as has hitherto been thought, but also that this yellow substance characterized by its solubility in alcohol and in water, owes this singular property to a particular matter which may be separated from it by well rectified ether; that this matter, insoluble in water, is soluble in boiling alcohol; that it is deposited by cooling, in crystalline plates similar to spermaceti; that it is soluble in any proportion in oils; and this last property agrees well with the daily practice of pharmaceutists in forming their epispastic preparations.

After having resolved the first question proposed, I endeavoured to ascertain the nature of this hitherto unknown acid; for this purpose, I undertook a new set of experiments, which I shall now proceed to relate.

I commenced by putting cantharides to infuse in cold distilled water; twelve hours afterwards I filtered this solution; it was of a deep brown red colour, reddened turnsole, and coagulated by heat, the coagulum resembling the ordinary scum of meat. Being again filtered, the infusion, always acid, afforded by means of lime water, an abundant floccy precipitate. The oxalate of potass had nearly the same effect; with acetate of lead the precipitate was very considerable.

Of all the re-agents I employed, ammonia the most engaged my attention, because it led to the most satisfactory results. I have already said, that the infusion of cantharides was acid,

and it happens that when this acid is saturated by volatile alkali, there is instantly formed a crystalline precipitate rather yellowish; after the saturated infusion has stood quiet for a few hours, there is no longer any precipitate formed by lime water, as took place before. We must conclude from this experiment, that the phosphoric acid, whose presence in the infusion was indicated by the lime water, makes a part of the precipitate obtained by the saturation, because this acid is no longer to be found in the liquor; and as, on the other hand, the ammonia takes up only the uncombined acid, it necessarily follows this cannot be the phosphoric acid, as M. Beaupoil had conjectured.

This crystalline precipitate, when rubbed with caustic potass, gave out a great deal of ammonia; it dissolved very readily in distilled vinegar, and upon adding to the solution a few drops of acetate of lead, a white precipitate was formed; upon cooling, it took the polyhedron form, which characterizes the phosphate of this metal. Hence it appears, that a triple phosphate is precipitated from this infusion by means of the volatile alkali. In order to determine the other base of this triple phosphate, I took a certain quantity of it, and calcined it, to drive off the ammonia; I then treated the residue with sulphuric acid; the solution, which was perfect, except a few floccules, being evaporated, there was left a gelatinous mass, as if it contained a great deal of silex. Thinking that the excess of sulphuric and phosphoric acids which was contained in it, might, by their mutual affinity for the same base, prevent the crystallization, I took up this excess by alcohol, and the residue treated by water, gave a prismatic salt of a bitter taste, having all the properties of sulphate of magnesia. Thus it may be regarded as fully demonstrated, that the infusion of cantharides contains phosphate of magnesia, which is held in solution by an uncombined acid, different from phosphoric, and that this phosphate is precipitated in the state of a triple salt by adding ammonia. This result, true as it is, will appear the more singular, as M. Beaupoil in his analysis of cantharides, makes no mention of this salt, although it is found in them in great quantities.

It is possible that this magnesian salt formed a part of the

bony frame of the cantharides; but if their bones have so little density, as to permit the phosphate of magnesia to be taken up by the acid even in the cold, how does it happen, that the phosphate of lime, which is the basis of them, and which is so abundant in the remains of cantharides, when treated by water and alcohol, is not equally dissolved, although it is not quite so readily attacked?

It remains now to ascertain the nature of the acid contained in the infusion of cantharides, but it may be easily conceived, how difficult it is to separate it from all the substances with which it is mixed. I have often thought I had separated it by means of different re-agents, but on examining the products attentively, I perceived my error; at last, after many ineffectual trials, I succeeded in obtaining it by a very simple method.

It is sufficient indeed to infuse bruised cantharides in ether at 60° of heat; this becomes of a dirty yellow colour after two or three days' maceration. This ethereal tincture is to be decantered and poured into a shallow capsule; the ether in evaporating leaves a reddish yellow oil, and when it is entirely dissipated, there remains a little colourless fluid separated from the oily matter. This liquor strongly reddens turnsole, is not precipitated by any re-agent, and affords by distillation an acid product possessing the same properties; by these characters, I recognised the presence of vinegar. It is almost needless to say, the same ether afforded no acid by its evaporation, when it had not been macerated upon cantharides.

Fearing lest the acetic acid, obtained in the preceding experiments, should proceed from the vinegar commonly made use of to kill the insects, I took some live cantharides, which had never been submitted to the action of any foreign substance. After bruising them in a mortar, I immediately introduced them into a glass retort, and observing every requisite precaution, I distilled them in a salt-water bath. The product condensed in the receiver was a little milky, and strongly impregnated with the odour of cantharides, without any thing penetrating; it reddened turnsole paper, but this did not take place very speedily. The earthy and metallic salts produced no precipitate in this liquid. We find, therefore, that this product contains acetic acid, but in very small proportion, and,

moreover, that the infusions or decoctions of the recent cantharides afford as unequivocal signs of acidity as those of commerce. Hence arise new doubts, which require new experiments; I therefore took some of these fresh cantharides, and boiled them in distilled water; the filtered decoction was evaporated, and during its evaporation I remarked, that a sediment was thrown down of an earthy appearance, and which was much more abundant than what took place under similar circumstances from cantharides, which had been collected some time before. Before the decoction attained the consistence of thin syrup I stopped the evaporation, to separate the sediment, which I washed in cold water, and found to possess the following properties.

This sediment commonly appears in the form of a grained powder of a yellowish-gray colour; applied with a little water to turnsole paper, it reddened it; it grates under the teeth, and has a little taste; thrown upon burning coals, it diffuses an odour of animal matter; it is partly dissolved in boiling distilled water, from which it separates on cooling, in the form of gray floccules. The solution, when poured off from these floccules, affords a precipitate by the acids, and by nut-galls; alkalies, and especially lime-water, also produce a precipitate from it; some of the sediment in question being rubbed with caustic potass, it formed a soapy paste, which dissolved for the most part in water. The alkaline solution, being filtered, was abundantly precipitated by acids; and when the solution is in a very diluted state, and a considerable excess of acid is poured into it, the precipitate, which slowly takes place, affects a crystalline form. These small crystals dissolve in concentrated nitric acid with a great effervescence and separation of nitrous gas. The solution, evaporated to dryness, gives the fine rose colour which characterizes the uric acid. I am equally sure, that besides the uric acid, this sediment contains a little phosphate of magnesia, and a certain quantity of animal matter.

Although the presence of uric acid in cantharides has nothing surprising in it, yet it is, however, very curious that these insects, which produce so remarkable an action on the urinary organs, present in their composition so many points of analogy with the urine itself.

Before I conclude this article, I must remark, that cantharides long kept, do not furnish the same products. I could never discover any uric acid in them; and had I not many times obtained this acid from recent cantharides, I should have considered this result as being involved in some doubt.

I said, in relating one of the above experiments, that ether macerated upon cantharides, afforded, besides the acid I have mentioned, a reddish-yellow oily liquid. I treated this fluid in the cold with rectified alcohol, but as no effect was produced, I employed heat. This oily matter fell to the bottom, and united into globules without being dissolved; the alcohol was, however, slightly coloured. I poured it off, and upon cooling, it deposited a great quantity of micaceous plates, which proved to be the vesicating principle; it was united to a very small portion of greasy matter, from which it was disengaged by dissolving it several times in boiling alcohol.

As to the other substance, it is really a fatty matter, which has all the properties of the fixed oils; it is distinguished from the green oil, which I have before spoken of, by its insolubility in alcohol; but neither of them is at all epispastic. This is then to be regarded as an additional product in the analysis of cantharides.

I was much surprised by obtaining a yellow oil when ether was employed, which was never obtained by any other method; some experiments, however, have led me to believe, that this oil, although insoluble in alcohol, forms a part of the yellow substance; this fact, though it appears extraordinary, is only a confirmation of what we frequently find takes place in other cases.

As I have thought it of most importance to insist particularly upon some material circumstances, the better to understand the particular object of my present inquiry, I have not observed the order requisite for a methodical analysis; intending, in the second part of this Memoir, to describe the progress and detail of my experiments; which second part will also contain a particular analysis of each individual substance I have announced in this.

The great advantage which will result from this essay, is undoubtedly the being able, correctly, to appreciate the medical

virtues of each of the products of cantharides; to determine exactly to which of them any particular effect is owing; to discover which may assist the powers of the others in any circumstances; and lastly, what substances may increase or destroy their activity. These results, so highly important to the healing art, can only be obtained by a great number of experiments, requiring besides such an extensive knowledge, that I cannot flatter myself with being able to conduct them successfully. I therefore hope, they will so far interest some other person, as to induce him to undertake those parts which more particularly relate to medicine; and I have then no doubt, but these experiments will altogether form a very interesting work upon the subject.

On the Treatment of Gout.

By Mr. WANT.

[From the Medical and Physical Journal for September, 1811.]

Ὑπολῆψιν δε εκ τυτυ το παθος ανιαραν εκτησατο, και μηδε ὁλως ὑπο τεχνης ιατρικῆς ἰαθῆναι ποτε δυνηθῆναι· ἐγω δε φημι ὡς ει διαγνωσθειεν καλως αἱ τε διαφοραι και τα ειδη αυτης ὁσα τε και οια τυγχανη, ευθεραπευτος αν ῥαδιως ὑπο των ιατρων γινησεται.

"An opinion has been entertained that this disease is not curable by the medical art, but, I affirm, that if physicians make a proper distinction between the different species of the disease, it may *very easily be cured.*"—ALEX. TRALLIAN, περι ποδαγρας, lib. xi.

"Est enim remedium hoc tantæ efficaciæ, et hominem totum liberum a Podagra, et articulario morbo præservet, ita ut eo qui utitur, nihil ei opus sit de sanguine extrahendo aut remediis aliis, ut *longa experientia compertum habetur.*" LUDOVICUS ENRIQUES, De Podraga.

No subject of late has more engrossed the attention of the profession, and, perhaps, of the public at large, than the nature of gout, and the power of the French medicine in alleviating the paroxysms of that distressing malady. That a disease generally considered incurable should now be relieved, as it were, by a charm, could not fail to excite the wonder, and rouse the energies of the medical world. The effects of this medicine were no sooner acknowledged, than experiments were instituted in all quarters, as might naturally be expected,

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When first the medicine was administered …
when its effects were in a measure veiled in mystery, when more was attributed to it than subsequent experience proved to be warranted by fact, when it was supposed to possess properties totally unlike any other remedy in use, the inquiry was most natural. In the course of time, however, a long succession of facts has taught us so much of its operation as to supersede, in my opinion, the necessity of any new attempt to imitate it. Daily experience shows us, that a gouty paroxysm is curable by the production of certain sensible effects on the constitution. If any one medicine, therefore, or combination of medicines be discovered, which is capable of producing those effects, it is a fair inference that the gout may be cured with as much certainty in the one case as in the other, and the scientific physician will have no cause to regret his inability to compound a medicine, whose colour, taste, and smell, are similar to that which led him to the discovery. Under these circumstances it is only as a matter of curious speculation, that I should consider the farther prosecution of the inquiry as deserving attention.

Should Mr. Moore's opinion of this medicine, and the source from which it is derived, be supported by additional evidence, the profession will learn to set a higher value on the labours of antiquity than they have hitherto done. Their valuable repositories, in general, record the language of experience; and though much which they contain has been rendered useless by the efforts of succeeding generations, there yet remains a copious harvest for the industrious investigator.

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with the view of discovering its composition; but a period of nearly eighteen months had elapsed without any communication being made to the public upon the subject, until the appearance of Mr. Moore's Tract, in which he endeavours to prove its identity with a mixture of hellebore and opium. The reasonings in support of his opinion, as far as a cursory view of his paper has enabled me to judge of them, are conducted with much ingenuity; but whether he will succeed in establishing this point I shall not take upon me to hazard a conjecture. I must confess, indeed, that I attach less importance to this part of the inquiry than many may be disposed to give it. When first the medicine was administered in this country, when its effects were in a measure veiled in mystery, when more was attributed to it than subsequent experience proved to be warranted by fact, when it was supposed to possess properties totally unlike any other remedy in use, the inquiry was most natural. In the course of time, however, a long succession of facts has taught us so much of its operation as to supersede, in my opinion, the necessity of any new attempt to imitate it. Daily experience shows us, that a gouty paroxysm is curable by the production of certain sensible effects on the constitution. If any one medicine, therefore, or combination of medicines be discovered, which is capable of producing those effects, it is a fair inference that the gout may be cured with as much certainty in the one case as in the other, and the scientific physician will have no cause to regret his inability to compound a medicine, whose colour, taste, and smell, are similar to that which led him to the discovery. Under these circumstances it is only as a matter of curious speculation, that I should consider the farther prosecution of the inquiry as deserving attention.

Should Mr. Moore's opinion of this medicine, and the source from which it is derived, be supported by additional evidence, the profession will learn to set a higher value on the labours of antiquity than they have hitherto done. Their valuable repositories, in general, record the language of experience; and though much which they contain has been rendered useless by the efforts of succeeding generations, there yet remains a copious harvest for the industrious investigator.

From all that we can collect respecting the sensible operation of the gout medicine, it appears evidently to be a purgative of the drastic kind; and as most purgatives in an over dose evince an emetic quality in a greater or less degree, so upon this supposition it is easy to conceive that this medicine may, in many cases, produce nausea and vomiting.

Now the treatment of gout by strong purgatives is by no means a modern discovery. The limits of a paper of this kind will not allow me to trace back the history of the practice: it will be sufficient to observe, that it prevailed pretty generally and successfully previous to the time of Sydenham, when unfortunately for science and the afflicted sufferer, that enlightened physician interdicted the practice, because it did not coincide with his ideas of morbific matter, and the necessity of its expulsion through the medium of the extremities. "It being," he observes, "an inviolable law of nature that the matter of the disease should be thrown out by the extremities; emetics and cathartics will have no other effect than that of bringing back the offending matter to the bowels."*

In another part of his treatise, we are told that the practice, however dangerous, was in high repute with certain empirics; he admits that during the purgative operation of the medicines administered, the patient felt no pain, or at least very little; and that if the catharsis could be kept up for several days, the patient was certainly cured of that paroxysm.†

* "Deinde, catharsin quod attinet, sive ἄνω sive κάτω animadvertendum est, quod cum naturæ lex sit inviolabilis, atque ipsi hujusce morbi essentiæ intertexta innexaque, morbi fomitem semper in articulos rejici debere; nihil prorsus aliud præstabunt remedia sive emetica, sive cathartica proprie dicta, nisi ut materia peccans, quam natura in corporis extremitates protruserat, in sanguinis massam denuo revocetur; unde accidit ut quæ in articulos eliminari debuerat, in aliquod e visceribus forte irruat, atque ita æger, qui in nullo prius discrimine versabatur, jam de vita periclitetur." SYDENHAM, Tractatus de Podagra.

† "At vero hæc ipsa methodus, utut perniciosa ac nocens, nihilominus empiricis quibusdam, qui catharticum quo utebantur medicamentum astute omnes celarunt, haud mediocrem estimationem conciliavit. Observandum est enim quod purgatione currente, æger vel non omnino, vel remisse admodum, dolet; & si catharsis ad plures dies produci queat, nullo superveniente paroxysmo recenti, æger confestim ab eo, quo jam tenetur, convalescet: verum enimvero pœnas in posterum pendet dirissimas ab ἀταξίᾳ, in quam dicta humorum exagitatio naturam præcipitem egit."

That this theory was framed in the face of personal experience, may be inferred from the latter part of the quotation, which amounts to an acknowledgment of the utility of the practice. That it was in opposition to the best practical authorities the world ever produced, will be evident to any one who will take the trouble to investigate the subject. Dr. Cheyne, says, "that in his time some eminent physicians had so little regard to the opinion of Sydenham in this matter, that in the fit of the gout itself *they never scrupled to drive it off*, both in themselves and others, by strong, quick, and active purges;" he further adds, "and most certain it is that this method will cure any fit of the gout, how obstinate soever, and that in a few days." We have also the authority of Kirkland for this practice, who asserts, that he has never yet seen under an inflammatory state, any method equal to purging for giving immediate relief; and this remark extends to the disease when it has attacked the stomach and bowels.

Scammony is a medicine which has been famed in all ages for its power of subduing the paroxysms of gout; and it is remarkable, that I have met with no author on the subject, whose prescriptions are not found to contain a large portion of this drug. The electuarium caryocostinum, the active material of which is scammony, was sold, in the time of Tissot, as a quack medicine in Geneva, under the title of opiate for rheumatism, and which he highly extols with some exceptions as to particular constitutions. Bate recommends a tincture of scammony as a medicine of great service in the gout, and the same tincture is, at this time, a popular remedy for that disease in France; and I am credibly informed that one gentleman for thirteen years past has, by means of it, succeeded in removing the paroxysms whenever they occurred.

Salmon extols scammony and elaterium as medicines which "*admirably help the* gout," and Turner's arthritic powder is a composition of turbith, hermodactyls, scammony, senna, and elder seeds, and was considered in its day as almost a specific for this complaint.

Willis recommends the electuarium caryocostinum and hermodactyls, which, he says, will make such as are not able to go, presently to walk about. The same medicine was before

prescribed by Alexander, with the same expressions, as to its virtues, so as to leave little doubt of its having been taken from that author.* He quotes Rodericus a Fonseca, in recommendation of the roots of black hellebore, and amongst other things an apple, with ℥ss of its fibres stuck in it, which is to be roasted under the embers and eaten.

It would be easy to multiply authorities if it were necessary, but as they are within the reach of every inquirer, I shall merely add the following extract from Hoffman, which, from the information it contains, appears of too much importance to be omitted.

"Hisce deductis operæ pretium esse æstimo paucis judicium nostrum interponere de methodo hactenus usitata, sanandi dolores podagricos. Plures et ex iis exercitatissimi practici vomitoriorum usum non satis dilaudare queunt in affectu hoc, tam preservando quam sanando. Confert, ut podagrici omni verno et autumnali tempore circa æquinoctia preservationis causa assumunt vomitorium. In principio quoque podagræ, proficuum esse ipsi Galenici jamdudum testati sunt. Dolæus Encyclop. Med. p. 635. Hildanus, Cent. 6. Obs. 34. Mercatus de Morbis internis, L. 4. c. 18. Riverius praxi, L. 6. c. 1. Sylvius, pr. p. 153. Emeticorum usum præsentissimum judicant. Alii magni faciunt preservationis causa laxantia ante ipsum paroxysmum, vel singulis mensibus.

"Doctissimus Solenander in Cons. p. 78, scribet se in paroxysmi principio solere exhibere, magno cum successu, aliquod purgans; primum id edoctum ab Alex. Tralliano, l. xi., licet a multis hoc tempore reformidatus. Singulare esse scribit eruditissimus Pechlinus, Obs. 26, quod arthritici leviorem plerumque experiantur insultum, detracta ex primis viis materia. Novi, ait, Empiricum qui medicamento chymico, seu Chrystalli Lunæ essent seu Martis aliquod vitriolum, etiam in inveterato jam morbo postquam biduo ante paregoricum de-

* Alexander's prescription is composed of hermodactyls, ginger, cummin seed, pepper, anniseed, and scammony, which makes those who take it walk immediately.

"τυτο ευθεως βαδιζειν αυτοις ποιει."

ALEX. TRALL. IN LOCO.

disset, alvum magno cum fructu provocabat. Ex adverso Sydenham illorum usum penitus rejicit. Nostra sententia hæc est; qui plenius vescuntur, et largiori victui assueti sunt, et mox ante paroxysmum primum invitante appetitu plus justo indulserunt gulæ, appropriata cathartica vel emetica summo cum fructu atque emolumento assumere possunt. Depletis sic primis viis a copiosa saburra humorum et acidi superantis, leviores evadunt paroxysmi, nec tam molesta symptomata anxietates dolores patiuntur ægri, &c. In eandem sententiam nobiscum accedit Lister, Tract de Arthritide, qui ita loquitur, &c."*

In another treatise, de dolore podagrico et arthritico, he says, "Memini me olim statim sub initio doloris podagrici et arthritici, insigni sæpe cum fructu, usum fuisse sequentis mixturæ. Aq. flor. acaciæ ℥ij. Aq. cinn. sine vino ℥ss. P. corna-

* Dissertatio de genuino et simplicissimo podagræ remedio. Hoff. oper. vol. vi.—The sense of the author may be thus rendered:—"I shall say a few words on the method hitherto employed for the cure of gouty pains. Many of the most experienced practitioners speak in the highest terms of emetics in this complaint, both for prevention and cure. Gouty patients derive great benefit from emetics taken as prophylactic, at the vernal and autumnal equinoxes. In the commencement also of gout, the beneficial effects of these remedies have been long ago testified by the followers of Galen. Dolæus, Hildanus, Mercatus, Riverius, and Sylvius, consider emetics as a most effectual remedy. Others think highly of laxatives as a means of prevention before the paroxysm, or taken monthly.

"The learned Solenander writes, that he was in the habit of administering a purgative with great success, in the commencement of a paroxysm, that he first learned this practice from Alexander of Tralles, though by many it was much dreaded.

"Pechlin says, 'it is a singular fact that arthritics have a milder fit if the primæ viæ have been first cleared. I know,' says he, 'an empiric who, by a chemical medicine, whether it was the crystals of silver, or a vitriolate of steel, is uncertain, procured evacuation from the bowels with great success, and this even in an inveterate disease, he having given a paregoric two days before. On the contrary, Sydenham entirely rejects the use of purgatives. I am of opinion, that in those patients who are accustomed to eat much, and who just before the paroxysm have been too indulgent with their appetite, proper emetics or cathartics may be taken with the greatest advantage.

"The primæ viæ being thus cleared of the saburra of the humors and superabundant acid, the paroxysms become milder, nor do the patients feel so much pain or uneasiness. Lister, in his Treatise on Gout, is of the same opinion with me on this subject, and has these words, &c."

chini ʒj. Syr. rosar. laxativ. vel rhabarb. cum cichoreo ℥ss. M. prohaustu. A drachm of the pulv. cornachini contains, scammony, diaphoretic antimony and cream of tartar, of each equal parts, so that twenty grains of scammony was given for a dose.*

The experience of the present day is equally strong in favour of the practice.

Very early during the agitation of this question the attention of the profession was generally directed to the Elaterium as a medicine most likely, from its known effects, to be the basis of the *Eau Medicinale*. I determined, therefore, to put it to the test of experiment on the first patients I met with. Three cases soon presented themselves, in which the curative powers of this remedy were as speedy and as satisfactory as any thing that could possibly have been expected from the gout medicine itself.

It is deserving of remark, that independently of its purgative, emetic, and diuretic effects, Elaterium possesses another property in common with the gout medicine, that of producing a tingling sensation in the extremities of the fingers.

This effect, which was noticed in the year 1695, by Dr. Martin Lister, was first pointed out to me by Dr. Buchan, of Percy-street, and was very strongly marked in the persons to whom I administered it. Whether this property is only exerted in gouty patients must be determined by future observation; Dr. L. however, spoke of its effects generally in his Treatise on Dropsy.†

The first case which was submitted to its influence was that of a labouring man, John Tomkins, who had long been a sufferer from that form of the disease called by many rheumatic gout,‡ and was never entirely free from its attacks; he had

* "I remember having used the following mixture with great success in the commencement of gouty pains, &c." De dolore arthritico et podagrico. Hoff opera, vol. 2.

† Lister Exercitationes de Morbis Chronicis. Tract. de Hydrop. p. 50.

‡ Some practitioners call the acute rheumatism, rheumatic gout; the latter disease, which is here alluded to, is characterized by inflammatory swellings in the larger joints of the body, *with little or no fever*, whereas acute rheumatism is always attended with great heat of skin, and the other symptoms of fever.

been under the care of the most eminent hospital physicians in town, without deriving any benefit from their treatment. I recollect having attended him upon a former occasion under a severe illness from this complaint, when his life was despaired of for many days, so much was he reduced by its violence and duration; at this time, the stomach and bowels were much affected by metastasis, and he recovered under the use of large doses of volatile tincture of guaiacum.*

He had now inflammatory swellings of several joints, particularly of his hands, wrists, and feet; the knuckles were permanently enlarged, and the fingers in a state of contraction, from former attacks. The pain often subsided in one part for another; sometimes it went from the joints to the bowels, and his friends became seriously alarmed for his safety. I gave him half a grain of elaterium every two hours, and on the following morning I found him free from all his symptoms, but great debility, and complaining of the severe operation of his medicine. The disease returned the following day, and was again relieved in the same manner.

Within these few days, a period of nearly eight months having elapsed since his last illness, he has had another attack of the gout; but here the seat of the disease was not as in the other case in the extremities, but confined to the bowels. He had been ill nearly a fortnight, complaining of tightness across the bowels, and what he denominated a heavy pain; he had nausea, impaired appetite, and flatulency; his bowels were costive, and he was generally indisposed. These were considered to be symptoms of gout, affecting some of the abdominal viscera. Three grains of elaterium were ordered, with directions for him to take one half immediately, and the remainder at night. Having taken one portion of this medicine, in the evening he was attacked with the most violent pain in his bowels; the remaining portion was given him, and a purgative clyster with two drachms of aloes and the same quantity

* A more extended experience, since this time, has abundantly confirmed me in the opinion I had formed of the efficacy of this remedy when administered in large doses. I think I may assert with confidence, that there are few cases of gout which may not be subdued by means of it. The efficient dose is ℥ss or ʒvj taken in half a pint of water gruel, night and morning.

of jalap ordered, which however was not administered. On the following morning I found he had been in excruciating pain all night; in the evening I saw him, the tongue was of a reddish brown colour and parched, his breath offensive in a remarkable degree, of which no words can convey an adequate idea, but which is very commonly met with in this stage of the complaint. With these symptoms of evident danger I did not think it right to rely solely on the elaterium; three grains more were given, and ℥xvj of blood ordered to be taken away; he was easy for the space of three hours after the bleeding, when the pain returned; on the following morning he was in great pain, the medicines had not operated. The clyster was repeated twice this day without effect; in the evening he was still in acute pain, and was again bled; ℈j of calomel was given, with directions to have the clyster repeated, which however was omitted; he was much relieved by the bleeding, but on the following morning the pain returned; he had one stool in the night; half an ounce of volatile tincture of guaiacum with fifty drops of laudanum were now given, which produced ease for the whole of the day. Towards evening the pain returning, the medicines were repeated with equal benefit. On the following morning he was free from pain, and his bowels were open; he was directed to continue his medicines a day or two longer, and is now quite recovered.

In the other cases the disease was confined to the back of the hand and wrists, it had here existed many days; each had three grains of elaterium administered, which produced a strong purgative effect, but the symptoms were removed with the exception, in one case, of a trifling degree of pain still remaining, though it was not of sufficient importance to require a repetition of the medicine. In one of these patients it was remarkable that the pain ceased before the evacuation from the bowels took place; it is not improbable that the salutary operation of the medicine was occasioned by some change in the secretions of this viscus effected by it. The same result is sometimes observed from the eau medicinale.

These cases have been related to show the advantage of attacking this complaint by purgatives. The precursory symptoms of a paroxysm of gout; the inappetency, flatulence, sour

eructations, and constipated bowels, indicate a disordered state of the intestinal canal. If the village dame be consulted by a patient in this case, he will be told that his bowels are in a bad condition, that he must take a dose of physic; and, probably, the opinion of the medical practitioner will not be very different, if he has not the good sense to know that gout is coming on. But if his mind is warped by prejudice this knowledge is sufficient for the prohibition, and the unfortunate sufferer is consigned to patience and his flannels.

The *sicca alvus*, or torpor of the intestinal canal, is a very common attendant on this disease. The celebrated father of physic, who lived 360 years before Christ, thought that gout was curable by no human means in old men, with *constipated bowels* and nodosity of the joints. But the disease even in them was not proof against a providential attack of dysentery, which cured it readily. He found other profuse evacuations from the bowels (εκτηξις, which literally signifies a melting down) also very useful; and he tells us, that a young man whose bowels were readily acted upon, who lived regularly, and was fond of labour, had only to find a physician with common understanding, and he might easily be cured.*

The power of diarrhœa in carrying off a fit of the gout when seated in the extremities is so notorious, that it could hardly escape so accurate an observer as Sydenham; but blinded as he was by a prepossession for his favourite opinion, he could not be expected to derive that practical advantage from it which the fact was capable of imparting. We cannot, then, be surprised to find him giving solemn directions for its suppression, as the only means of recalling the wanderer to his proper seat.

"There is but one remedy," he says, "that I know of, viz. to produce perspiration, by the medicines usually given for this

* Ὅσοι μεν ὦ γερόντες, η περι τοῖσιν ἄρθροισιν επισωρωματα ἔχυσιν, ἢ τροπον ταλαιπωρον ζωσι κοιλιας ξηρας εχοντες ουτοι μεν παντες αδυνατοι υγιεες γινεσθαι ανθρωπινη τεχνη. οσον εγω οιδα· ιωνται μεν τουτυς, αριστα μεν δυσεντεριαι ην επιγινωνται, αταρ ϗ αλλαι εκτηξιες οφελυσι καρτα, αι ες τα κατω χωρια ρεπυσι· ος δε τις νεος εςι ϗ αμφι τοισιν αρθροισιν υπω επισωρωματα εχει, ϗ τον τροπον εςιν επιμελης τε ϗ φιλοπονος ϗ κοιλιας αγαθας εχων, υπακουειν προς τα επιτηδευματα, ουτος δη ιητρου γνωμην εχοντος επιτυχων υγιης αν γενοιτο. Hippocrates. προρρ. lib. 2.

purpose, which if effected for two or three days, morning and evening, two or three hours at a time, the diarrhœa is commonly stopped, and the gout comes *thundering* back upon the extremities."* It should be remarked, however, that when he has reason to believe the diarrhœa is critical, he does not attempt to interfere with it.

It is impossible for the imagination to contemplate the strength of the *thundering* figure, which he uses to express the return of-gout to the limbs, without picturing to itself the triumphant ecstacy of the man on the seeming victory he had obtained; whilst his patients, writhing under the most insufferable anguish human nature is capable of bearing, are consoled with the cheering prospect of longevity, which he invariably calculates upon, *cæteris paribus*, in a certain proportion to the violence of the pain.†

How different is the conduct of Musgrave on this point, who, after telling us, that during a paroxysm of gout a diarrhœa often takes place, which carries off the pain and tumefaction,‡ adds, that "the diarrhœa which *anticipates* the fit is frequently found to be salutary, health and vigor returning after it.§ The event of it, however, he confesses is uncertain. If it stops in time and is not *excessive* it proves serviceable, by carrying off the gouty matter by a safe way, though not the most common; and it has this advantage attending it, that the paroxysm *does not return for a long time after*.|| He displays

* "Unicum, quod scio, remedium est, ut sudor provocetur methodo et medicamentis huic usui destinatis: quod si fiat ad biduum triduumve mane et vesperi, per duas tresve horas continuas, sistetur ut plurimum diarrhœa, et morbi fomes magna vi in artus *detonabit*." SYDENHAM.

† Morbo jam discusso, ægri tum ευεξια, tum appetitus, redeunt, prorata doloris quo sæviebat paroxysmus nuper elapsus, et in eadem proportione servata, vel acceleratur vel differtur sequens paroxysmus. Nam si hic ultimus ægrum *pessime multaverit*, sequens paroxysmus non, nisi anno ad idem punctum revertente, denuo accedet." SYDENHAM.

‡ "Accidit diarrhœa; eamque statim excipit doloris et tumoris diminutio." De Arthritide Anomala, page 137.

§ "Atque istiusmodi fluor paroxysmos interdum Arthriticos antevertens, salutaris esse reperitur. *Expulso etenim inimico, clauduntur januæ; pax, et quies restituitur; viget œconomia*." Page 138.

|| Hujusmodi diarrhœa incerti admodum eventus est; si enim tempori cohibeatur, neque modum excedat, utilis invenitur; et quidem arthritidis

the folly and danger of an attempt to stop it; "for an officious diligence," he says, "disturbs nature, and interrupts her in the work she has begun, when it is better to leave her to herself, and permit that to be discharged, which if retained would do mischief; but if it becomes excessive it should certainly be restrained."*

That the gout is symptomatic of the affection of the bowels, I have little doubt. Stahl endeavours to account for the alternation of this disease with hypochondriasis, which may evidently be traced to this source.† If we admit that erysipelas is often connected with sordes in the intestinal canal, we shall have little difficulty in conceiving that another inflammation, in some respects similar, may arise from the same cause.

The affinity that exists between these two diseases may be inferred from the fact of their frequently alternating with each other. After a paroxysm of gout has been fairly fixed upon a joint, in all regularity and form, and has continued there for several days, it is sometimes suddenly transferred to the face and neck, or other parts, where it appears in the shape of an erysipelas, and *vice versa.*

Hoffman relates the case of a patient who had an extensive erysipelas of the right leg, which soon seized the metatarsus and toe of the left side, with considerable tumefaction and pain. The surgeon, who was in attendance, applied spirit of wine and camphor to the foot, upon which it attacked the other: by the application of powdered camphor, the disease was again immediately relieved, but the patient was seized with symptoms of gout in the vicinity of the heart, with so great a constriction upon the diaphragm that he could hardly breathe.‡

materiam etiamsi via non usitatissima, tamen tuta emittere. Cui isthuc consequens, quod paroxysmus *non nisi diu post* recurrit arthriticus. P. 139.

* Nihil inepta sedulitate *stultius* dicam an *periculosius?* quæ, Pharmacis perperam ingestis, naturæ opus sæpe interturbet, interdum abrumpat. Suo potius arbitrio ea, patiaris, agat; quodque intus læsurum erat suo utiliter ingenio effundat, si vero diarrhœa *nimium excurrat,* viresque excedat, astringentibus sistenda est. Page 139.

† Stahl. Theoria Medica, page 1372.

‡ Observations Interessantes sur la Goutte et Rhumatisme, traduites de Fred. Hoffman. Obs. 2. 241.

Musgrave also speaks of the transition from erysipelas to gout in the joints;* but he is not warranted, I think, in assuming from this fact the existence of an arthritic erysipelas. The *morbi fomes*, the daily accumulations of indigested sordes, is the exciting cause of both these and of many other diseases. Gout in the joints, erysipelas in the skin, asthma, phthisis, peripneumony in the lungs, cholic in the bowels, commonly called gout in the stomach: these, and a variety of other symptoms from the same cause, may be removed by the evacuation of the offending matter, or by retunding and neutralizing its acrimony. His book is a most invaluable production, inasmuch as it shows what an endless variety of nervous and chronic diseases may be traced to this source.

If this opinion be established, the superiority of purging over bleeding will be evident; the latter may relieve, as in all cases of inflammation it does, but it is only by removing the cause that we can expect a removal of the disease.

With these facts before me, I cannot help recurring to the exploded doctrine of the humoral pathology, as affording the only plausible solution of the phenomena of this malady. Is it, I would ask, inconsistent with the known laws of the animal economy, to suppose that an acrid matter may be carried by absorption from the intestines into the circulation, and that its existence in the blood is incompatible with the healthy functions of the body? If I might be allowed to step beyond the limits of fair induction into the field of hypothesis, I would imagine the joints, from some peculiarity unknown to us, to be the parts chosen by nature for the deposition of this matter; or I see no difficulty in allowing its transpiration to be going on through every pore, whilst the joints or parts attacked, from this unknown peculiarity, are more susceptible of diseased impression from its acrimonious nature than others.

Pain is not an inseparable concomitant of this disease: its intensity, or indeed its very existence, is probably determined by the texture or sensibility of the parts attacked. Thus in the

* "*Faciei* porro *erysipelas* notavi quod detractione sanguinis, factaque inde materiæ in artus Διαδοχῆ quum in *dolorem* mutaretur isthuc *arthriticum*, naturam plane confessum est *arthriticum*, &c." MUSGRAVE de Arthritide anomala, p. 459.

commencement of a regular paroxysm which is seated in the ligaments of the joints, acute pain is felt, but when at the close of each "*paroxysmulus*" the pain is commuted for swelling, erythematous redness, and itching of the surrounding integuments, it is not unfair to conclude that the matter has been translated from the ligaments to the skin. When the disease by metastasis or otherwise affects the brain, separate from its meninges, no pain is felt, but giddiness, stupor, or apoplexy are the consequence.

Cullen's fourth objection to the theory of morbific matter is, that its operation, if it exists, should be similar in the several parts which it attacks; whereas it seems to be very different, being *stimulant*, and exciting inflammation in *the joints*, but *sedative*, and destroying tone in *the stomach.**

Lest any doubt should arise of the truth of this position, from the natural insensibility of parts of a ligamentous structure, I must be allowed in digression to observe, that however insensible in their healthy state, when they have taken on inflammatory action the pain is most *acute*. This arises from their dense, firm, and unyielding nature.

The acknowledged efficacy of blisters† in the cure of gout in many cases, when applied to the inflamed joint, strongly favours the theory of morbific matter. No person in the right possession of his senses would think of curing inflammation by means of blistering, unless with the hope of exciting a discharge of some irritating material producing the disease.

The local application of euphorbium,‡ elaterium,§ and me-

* First Lines, sect. 529.

† Καὶ ἄλλον ὁμοίως ἐθεασάμην κεχρημενόν τῶ διὰ κανθαρίδων φαρμάκω, ϗ ηὐχαρίστει τὰ μέγιστα, ῥηγνυμένης γαρ της γινομένης ὑπὸ τῦ φαρμάκυ φλυκταινης ὑγρὸν ἐξικρίνετο πολὺ ϗ τύτο συμβαίνοντος ἔφασκεν ὠφελεισθαι τὰ μέγιστα.

"I have seen another patient who, by means of a medicine composed of cantharides, has been cured, for when the vesication occasioned by it was ruptured, there came forth a great quantity of humor, which he said relieved him greatly." Alex. Trallian.

‡ Δόκιμόν ἐστι τὸ φάρμακον ϗ ἐπι πολλῶν πολλάκις εὐδοκιμῆσαν ἐπι τῶν τοιύτων διαθέσεων ϗ γαρ διαφορεῖ ϗ ἀμυσσει την επιφανειαν, ἕλκει τὲ ἐκ τῦ βαθυς τα ἐστηριγμενα, ϗ διαλυει ϗ την ὀδυνην ἐκκοπτει.

"It is a noble medicine, and frequently tried with success in this species of the complaint, for it excoriates and removes the cuticle, it draws forth that which is deep seated, and relieves the pain." Alex. Trallian.

§ "Radix elaterii ex aceto cocta podagricis illinitur succoque dentium

dicines of this kind, the beneficial effects of which are attested, the former by Trallian, the latter by Dioscorides and Pliny, can hardly be accounted for upon any other supposition.

Another reason in support of this theory, to say nothing of the relief demonstrably given by purgatives, is the immediate benefit arising from the use of those medicines which have the power of neutralizing acid secreted in the stomach and bowels. Half an ounce, or a larger dose of volatile tincture of guaiacum, given night and morning, is a remedy which from much experience I can recommend as a specific in many cases of this disease.

If the medical reader requires more evidence than his own experience will afford him of the existence of this acid, I may refer him, amongst others, to a case of gout related in the first volume of the Medical Observations and Inquiries, where a severe paroxysm was critically carried off by the vomiting of a considerable quantity of fluid, which from its acrimony was compared to strong mineral acid; added to this, the close analogy which subsists between some forms of dyspepsia in which the presence of acid is indisputably certain, and those symptoms which precede an attack of gout, leave little doubt of its existence also in the latter case.

In a systematic treatise much more might be adduced in support of this opinion; but as I fear I have already trespassed too long upon the patience of the reader, I must remain satisfied with having merely laid the foundation for future inquiry.*

dolori medetur." PLINII Hist. Nat. de Elaterio. Dioscorides is literally copied in this quotation.

* The objections to this doctrine first started by Stahl, and afterwards urged by the celebrated Cullen, are of little weight in the scale when opposed to the host of evidence which may be adduced in its favour. Cullen says, we have no direct evidence of the existence of morbific matter in the blood, and that previous to an attack of gout there appears no marks of any morbid state of the fluids. Granted: but we have strong proof of its existence in the intestines; and we shall have more difficulty in supposing the possibility of its continuance in the bowels without being absorbed into the circulation, than in accounting for the mode of its getting there. The arguments against the existence of this matter, deduced from the various and contradictory accounts of its nature, are too futile to deserve a moment's attention. It can never be deemed an objection to the truth of an opinion, that its partizans have taken wrong measures in its defence. Hence the attack upon the doc-

If we admit this fact, the dangerous consequences of an indiscriminate attempt to cure gout by the application of cold water will be obvious. That in many cases the powers of nature are proof against the destructive tendency of this practice cannot be denied, but that fatal instances have occurred in consequence of its employment is equally certain. Upon this point Trallian judiciously observes, "that no astringent or repellent medicines should ever be had recourse to, until the bowels have first been freed of the sordes contained in them."*

That gout is curable by medicine and proper attention to diet and mode of living, without danger of inducing other diseases, I have long been convinced by great attention to the subject; and so fully persuaded am I of this fact, that I should feel no hesitation in affirming that it is so in every case, if the experience of one individual could afford a sufficient warrant for such an assertion.

The administration of purgatives forms a prominent, though not the only feature in the medicinal part of its treatment. Their employment we have shown to be warranted by facts; they have the voice of reason on their side, for when "the enemy is driven from the citadel, and the gates shut, peace and tranquillity *must* be restored." They have the sanction of the most unquestionable authorities of antiquity for their utility; but that they are capable of removing the disease in every case, is more than I dare venture to affirm. Many cases may, no doubt, occur, in which they may not only be inefficient, but dangerous. The discrimination of the practitioner must here

trine contained in his 6th objection, founded on the mistaken inferences of its existence, from the hereditary nature and contagious property of the disease, is equally absurd with that last considered. The only objection which remains unanswered is, that the theory is inadequate to explain its frequent metastases from one part to the other. Is Cullen's theory unimpeachable on this score? No. Admitting then that we are not sufficiently informed respecting it to be enabled to explain *all* things *at present*, let us take that which we have as an earnest of that which is to come; and having nothing more plausible to oppose to it, wait with patience until more facts, or some newly discovered analogy, afford the complete and desired explanation.

* Ἐγὼ δὲ φημι μηδὲ τοῖς στύφουσι ἢ ἀποκρούεσθαι δυναμένοις ἐπὶ τῶν πεπονθότων ἐθέλειν κεχρῆσθαι, μὴ πρότερον ὅλον ἀπερείττον ἐργασάμενον τὸ σῶμα. ALEX. TRALL.

be exercised. Elaterium, though in a proper dose a *mild* medicine, will destroy life as well as the Eau Medicinale, and it is only by minute attention to the circumstances of the case, that its employment can be regulated.

As to the identity of any preparation of elaterium with Husson's remedy, it is, as I hinted above, of too little importance to contend for. Yet, I may observe, that the marked coincidence in effect between these medicines, speaks as strongly in its favour as medical evidence can speak. What pharmaceutical process it should undergo to render it similar in external appearance to that medicine, I shall leave to the consideration of those who are more curious in researches of this nature than I am.

This is a point at which no man of science need be ambitious of arriving. The research is endless. The time which has already been employed in the inquiry, and the varied speculations that have been entered into respecting it, with so little success, will almost justify this conclusion. It appears to me to be a prostitution of talents, implanted in us by the great Author of our being for wiser purposes; for we are seeking for that, which when discovered will add comparatively nothing to the stock of our information.

If I have shielded myself under the authority of men whose names are venerable in the annals of Medicine, I trust I have not suffered myself, by a blind adherence to them, to be drawn into the track where experience does not also lead. The remarks here made might have received support from cases under my own observation; but it is easy to make a tale suited to any theory, and the narratives of some modern writers are so much in opposition to the daily experience of all practitioners but themselves, that I was unwilling to risk an imputation so disgraceful to a man of science.

It is with reluctance that I have been induced to launch out into theory; contrary to my original intention, I have insensibly been drawn into it. If my opinions are opposed to facts, I shall be most happy to cancel them and to acknowledge my error. I have no object in this investigation but truth, and if I have not yet found her, she shall be a welcome guest whencesoever she may come.

JOHN WANT,
Surgeon to the Northern Dispensary.

July 23, 1811.

A Reply to some Observations and Conclusions in a Paper just published in the second volume of the Medico-Chirurgical Transactions, on the Nature of the Alkaline Matter contained in various dropsical Fluids, and in the Serum of the Blood. By GEORGE PEARSON, *M.D. F.R.S. Physician in ordinary to their Royal Highnesses the Duke and Duchess of York, and their household, &c. &c.*

[From the Philosophical Magazine, for January, 1812.]

To Mr. TILLOCH.

SIR,

I WAS favoured a few weeks ago, by Dr. Marcet, the author, with the above named paper. In it I have the satisfaction to find many of the facts confirmed, and none contradicted, which I have published in the Philosophical Transactions 1809 and 1810, on Expectorated Matter and Purulent Fluids; except with regard to the alkaline impregnations. My experiments informed me that expectorated matters and pus contain potash neutralized by an animal substance, or by an acid destructible by fire. I likewise found, as I prosecuted my inquiries, that there is the same kind of alkaline impregnation in the blood, in the dropsy fluids, in the fluid effused by vesicating with cantharides, in the fluid secreted from the nose owing to a catarrh, and even in the urine. And as I did not find the soda alkali in a similar state, I concluded that hitherto this alkali had, probably, been mistaken for the potash. (See our preceding numbers.) In the ingenious paper, however, which has occasioned this reply, it is asserted that the alkali in combination with the animal matter is the soda; but it is inferred that potash is also present, not in the state I discovered it, but united to muriatic acid.

It would not be treating the public justly, if I did not say that the paper before me contains an inquiry conducted conjointly by Dr. Marcet the writer, and Dr. Wollaston; as Dr. Marcet represents, I allow very fairly, to enhance the credit of his statement. Considering the power of these allied opponents, the odds are fearful; but confiding in the assurance of lord Bacon, that induction by experiment equalizes* the

* Nostra vero inveniendi scientias ea est ratio, ut non multum ingeniorum acumini et robori relinquatur; sed quæ ingenia et intellectus fere exæquet.—Bacon's *Novum Organum*, § lxi.

mental faculties among different men, I shall with this palladium obey the summons to the arena—at the worst issue, with such adversaries it would be glorious even to fall in the struggle:

——— agimus proh Jupiter! ———
—— Causam; et mecum confertur Ulysses.

To enable the chemical public to judge rightly of the different conclusions, above declared, concerning the kinds and states of the alkalies existing in the animal fluids, the evidence of the opposing parties must be heard. The adverse party, however, have not attempted to invalidate my evidence, by showing that the conclusions are unjustifiable, but have merely exhibited their own experiments and conclusions. This mode of procedure, I apprehend, is not according to the laws of controversy; and it compels me to make a statement of at least some of the most decisive experiments for my conclusions, previously to the examination of the contravening evidence.

I. 961 grains of exsiccated sputum, on incineration and fusion, afforded 45 grains of saline substances consisting of 35 grains of cubical crystals of muriate of soda, and the rest were spiculiar and uncrystallized salt amounting to ten grains. These ten grains were separated for distinct examination. They manifested the properties of alkaline matter. On adding liquid tartaric acid to this alkaline matter also liquified, an effervesence ensued, with a precipitate of super-tartrite of potash only; "*certainly yielding no soda-tartrite of potash.*" With nitromuriate of platina a grain or two of this saline matter produced a reddish precipitate. Now if muriate of potash, and carbonate or sub-carbonate of soda had existed, the result must have been soda-tartrite of potash and muriate of soda; or tartrite of potash and muriate of soda. This latter result is not so probable as the former, on account of the very large proportion of alkali to any other possible salt. The quantities, too, were obviously sufficient for producing compound salt determinable by the eye unassisted with glasses.

II. By digesting 2500 grains of desiccated sputum in four pints of alcohol or spirit of wine, the clear tincture decanted from off the undissolved matter afforded on distillation 140

grains of resin-like substance; which manifested no alkaline properties, but it indicated slightly acidity.

A portion of this resin-like substance being mixed with liquid tartaric acid was subjected to distillation; but neither muriatic nor any other acid was disengaged. This I conceive shows that no muriate of potash existed.

Twenty-five grains of this matter were acted upon by successive affusions of nitric acid; and on boiling to dryness and ignition, the deflagration which took place produced a charcoal-like mass containing potash. Hence the alkali had been united to something destructible by fire.

According to computation, the 140 grains of resin-like matter contained 28 grains of potash united to matter destructible by fire, and 18 grains of muriate of soda, with an inappreciable quantity of ammonia and phosphoric acid besides the animal matter. The matter undissolved by alcohol, in this process, afforded by incineration and fusion a mass consisting of 23 grains of muriate of soda with a very small proportion of potash mixed with 23 grains of phosphate of lime, traces of magnesia, iron, and a sulphate; also a minute portion of utterly indissoluble vitrified matter. If potash had existed in union with muriatic acid, it must have appeared in the fused mass left undissolved after digestion in alcohol: but potash did appear in a naked state after ignition and fusion of the matter dissolved in alcohol.

III. By digesting 4000 grains of sputum in two pints of rectified spirit of wine, the same results were obtained, excepting that the resin-like matter contained a much larger proportion of muriate of soda and animal matter.

IV. Twenty ounces of ropy sputum by digestion in ten pints of distilled acetic acid afforded, by evaporation of the clear liquid separated from the coagulated matter, a soft extract. This extract deliquesced, partially, on exposure for a few days to the air; but it manifested no properties of alkali. By exsiccation, ignition, and fusion of a little of this deliquesced matter, it afforded an aqueous solution which precipitated abundantly super-tartrite of potash on adding tartaric acid; and a reddish precipitate fell on the addition of platina solution. Almost the whole of this extract being exsiccated was digested in rectified

spirit of wine, affording a blackish tincture after evaporation to dryness, which became liquid by 24 hours exposure to the air. It was almost entirely acetate of potash. I believe acetate of soda neither dissolves in alcohol nor deliquesces; but, independently of these properties, the alkali united was proved to be potash.

I shall call no other evidence from a great mass which remains in my published papers. If I were to follow the example of my adversaries, I should also not trouble myself to examine their evidence; but as the question cannot be decided without such an examination, I beg permission to perform this duty.

I. *Of the Fluid of the Spina bifida.*

In the ten printed pages of experiments on this fluid by Dr. Marcet, I can only perceive that there is evidence for the existence of an alkaline subcarbonate; yet it is said, "Soda may be inferred from the effervescence with acids." The alkaline matter was treated with alcohol; and thus it was separated from the muriate. The alcoholic solution being decanted and evaporated to dryness, a residue supposed to consist of acetate of soda was obtained, which weighed between 17 and 18 per cent. of the mass." Oxymuriate of platina produced no precipitate.

I remark, that the first result only shows the presence of charcoal acid. 2. The acetate of soda is not, I believe, dissoluble in alcohol, but it is well known that acetate of potash is so. However, if there be the authority of experiment for the dissolubility of acetate of soda in this mentruum, still the experiment is equivocal. It was easy for the adverse party to have decided this question by the test of tartaric acid, provided there was an adequate quantity of matter for the trial.

3. I remark, that there being no precipitation with the platina solution seems to me to prove nothing; as the whole quantity of matter treated could not reasonably be supposed to amount to more than a small fraction of a grain; too small for the detection of potash by means of the platina solution, or even probably by the more sensible test tartaric acid, which was not used. Yet the ingenious writer has not only inserted

soda among the impregnating ingredients of the fluid under examination, but also boldy denoted the proportion to the centesimal part of a grain. I shall in another part of this communication, I believe, demonstrate that this analysis does not warrant the statement of the composition of this dropsical fluid given in such precise terms: for, on the ground of cogent analogy, I cannot doubt that one or more ingredients are present, but not inquired for by experiment, nor enumerated. Hence, not only is the analysis objectionable with respect to the ingredients but the proportions. It is true, in a subequent part of the investigation the deficiency seems to have been perceived and acknowledged; but if so, it will not be an easy task to justify the publication of perhaps an inaccurate analytical statement, in opposition to my experiments which have not been refuted.

II. *Of the Fluid of the Hydrocephalus internus.*

A few grains of the saline matter of this fluid consisted of cubic crystals mixed with spicular and opake globules. The assertion is several times made, that the spicular crystals and opake globules were carbonate of soda—that most of the cubes were muriate of soda; but some of the smaller ones were found to be muriate of potash. The proofs for the assertion are from the two re-agents I employed in the same inquiry; namely, tartaric acid and platina solution for the potash; and "the carbonate of soda was indentified, not only by tests indicative of the absence of potash, but also by its forming rhomboidal instead of prismatic crystals, when treated with nitric acid."

Now, I apprehend our judges will deem this evidence unsatisfactory; and that much more decisive proofs will be reasonably expected. I beg permission to ask, whether or not the laborious experiments upon a large scale, which I instituted, to exhibit evidence of the exclusive existence of the potash alkali, are to be disproved by the rhomboidal figure of the crystals, in place of prismatic, seen perhaps only by a magnifying glass in the quantity of a grain or two dispersed over a comparatively extensive surface; and whether or not the absence of potash, indicated by tests operating upon minute quantities, is unequivocal evidence, and ought to counterpoise

experiments, with quantities affording products, of which no doubt can be entertained. I do not question the accuracy; but I hope it is proper to take a further objection against the competency of the experiment asserted, for the presence of soda and absence of potash. On the most important point which occurred in the inquiry, the kind of alkali existing in the fluids, I do conceive that more experiments, and particularly detailed, are necessary to effect the dis-proof of what I have published, and to command assent, that soda and not potash is present. Is it satisfactory to affirm, that soda was indentified, because the tests did not indicate potash? It is quite superfluous for me to say to such learned adversaries as I have the honor of addressing, that an experiment might have been instituted to have afforded unquestionable proof of the existence of soda. Such a proof would be the composition of a binate salt, possessing the known properties of a compound of soda and the acid employed.

With respect to muriate of potash, that this is present, is supported only by the observation of smaller cubic crystals among larger ones; otherwise it is a mere assertion.

My last argument is of a different kind from those above stated. If carbonate of soda in a large, and muriate of potash in a small proportion be present, on the addition of tartaric acid, it is obvious that it is scarcely possible to avoid compounding soda-tartrite of potash, and certainly muriate of soda. If the learned opponents had produced these compositions, I must have conceded, at least, that carbonate of soda existed; but still it would require other experiments to determine the state of the potash.

III. *Of the Fluids of Ascites, Hydrothorax, and Hydrops Pericardii.*

A saline mass amounting to 4·8 grains, obtained by the process above mentioned, exhibited clusters of crystals, partly cubic, partly octohedral, interspersed with others of a feathery or radiating appearance. The feathery saline matter effervesced briskly with acids, and yielded no permanent precipitate, either with tartaric acid or with oxymuriate of platina. The cubic crystals

and octohedral yielded precipitates with either of the two tests just mentioned.

I do not conceive that these observations authorize the adverse party to contravene my experiments and conclusions. I know from experience, that it is probable the feathery crystals even of potash would elude detection, on account of the minute quantity; there was however a precipitate, but not permanent. The question naturally arises, What was that non-permanent precipitate? I have no doubt the quantity was too small to enable the question to be answered even by the hands that performed the experiment.

But the cubic and octohedral crystals yielded precipitates with either of the two tests; and hence potash is inferred to exist united to muriatic acid. I again must appeal to chemical judges, to determine whether or not the conclusion is warrantable; for, 1. Here is no proof of muriate of potash. 2. It is not even certain that the precipitate was super-tartrite of potash. 3. Granting that super-tartrite of potash was produced, it remains to be proved in what state this alkali subsisted.

IV. *Of the Serum of the Blood.*

The saline matter procured from this fluid did not, with the platina solution, "produce a precipitate sufficiently distinct to be conclusive as to the presence of potash; but, by means of tartaric acid, a distinct though not abundant precipitate was produced." Further, with nitric acid this saline matter yielded crystals of a "rhomboidal form." Again: this matter dissolved in acetic acid, being evaporated to dryness, was treated with alcohol and again evaporated: "the residue, contrary to my expectation, exhibited traces of potash; but the same residue, with nitric acid, yielded rhomboidal, and no prismatic crystals were seen;" whilst "potash was easily discoverable in the residue, which had now lost its deliquescent quality." I wish to avoid repetition of objections already offered, although they are applicable in this place, and will only remark: 1. That I cannot admit the figure of such minute crystals, as a decisive property; but the kind of nitrate compounded might have been ascertained by the test of tartaric acid. 2. The dissolution of the

acetate in alcohol is the most conclusive experiment given in the paper before me; and it has produced apparent embarrassment. Even as performed it is pretty determinate, and might have become an *experimentum crucis* by prosecuting it a little further. We know that acetate of potash is dissoluble in alcohol, and there is no proof that soda united to acetic acid is present; even if such a compound be dissoluble in alcohol. It has been thought right, however, to assume an hypothesis, or more truly two hypotheses, to account for the potash in the menstruum of alcohol; viz.—To imagine that muriate of potash is present. 2. That it is dissoluble in alcohol. If potash was present in the indissoluble residue, it was most important to have exhibited the state in which it existed. It was not difficult to determine, if doubted, the state of the potash in the alcohol, by burning the residue left on evaporation, which would have denuded it if united to the acetic acid, but not if united to the muriatic acid. Supposing it be judged right to receive these experiments as evidence of the facts asserted by the adverse party, I beg to claim the right also of opposing the contravening evidence above delivered, in stating the results of similar experiments.

From this representation, I submit to our judges, whether or not I am entitled to object to the enumeration of subcarbonate of soda as one of the impregnating ingredients of serum; and especially to the proportion denoted in centesimal parts of a grain, in a mass amounting to seven or eight grains, consisting of seven different substances. Having communicated merely the information of the senses* through the intermedium of experiments, the chemical world will determine whether or not the opposing party have demonstrsted errors in observation or illegitimate conclusions. I am of opinion, that the best founded conclusions are but provisional, and of course, that chemistry has not yet attained the rank of a science, or at least not of a demonstrative science. This opinion seems just, from

* Sensus enim per se res infirma est et aberrans; neque organa ad amplificandos sensus aut acuendos multum valent; sed omnis verior interpretatio naturæ conficitur per instantias et experimenta idonea et apposita; ubi sensus de experimento tantum, experimentum de natura et re ipsâ judicat.—Bacon's *Novum Organum*.

a retrospective view of the varying states of chemistry during the last hundred years. Many of the theories of the illustrious Stahl were for half a century admitted as demonstrations of the agency of phlogiston. That these doctrines were erroneous, was evinced by the succeeding discovery of the agency of oxygen, especially manifested by the ever to be lamented Lavoisier. And the pneumatic doctrine in some parts has lately been rendered doubtful, if not exploded, by the wondrous achievements of professor Davy. Contemplating the prospect of the progression of this branch of natural knowledge, I offer the conclusion, that potash, and not soda, is the alkali united to animal matter in the fluids I examined, merely as provisional. That potash does also exist in these fluids, united to muriatic acid, is not inconsistent with my experiments; but the experiments of my learned friends do not appear to authorize such an inference. The discovery, however, will be partly due to them if hereafter the fact be substantiated.

I cannot close this communication until I shall have said a few words concerning the high encomiums on microscopic chemistry, accompanied by the bitter philippic against "the dismal, large, subterraneous laboratory." Chemistry must now, we are told, be transferred to "the comfortable fire-side of the drawing-room;" from Vulcan's foul stithy to my lady's chamber. This *elegant* change is to give "new impulse to the advancement of the science, and new schools are to rise under new auspices." Most happy shall I be to find these Eutopian prospects realised. It seems, however, more than probable, that the successful impulses already given by the schools of "my very learned and approved good masters," Cullen, Black, and Fordyce, will retain the cultivators in the paths now opened. And with regard to the scene for operations, the privilege of *taste* will be asserted; for that indeed is not disputable either in chemistry or elsewhere. Becher's taste was opposite to that of the ingenious new advocates: "Nec quicquam præ carbonibus, venenis, fuligine, follibus, et furnis valere potest."—*Phys. Subter. Præf.* The lord high chancellor of England not long ago declared in court, that he would not pay *sixpence* for the rapturous notes of Mara or Catalani. This also was a matter of taste, and no one disputed: it was only observed by a large

majority, that his lordship had "no music in his soul, and was not charmed by concord of sweet sounds."—No more.

The value of a tree is best known by its fruits: and accordingly to inform the judgment of the public by practical examples, and as some return for the notice with which my papers have been honored, I shall, with your permission, offer for your next number a few remarks on the publication in general which has produced this communication; in which, whatever differing opinions may subsist, I assuredly must admire the ingenuity, and respect the knowledge, of the honorable antagonists.

G. P.

George-street, Hanover-square,
Jan. 14, 1812.

Experiments on Muriatic Acid Gas. By J. Murray, *Lecturer on Chemistry, Edinburgh.*

[From Nicholson's Journal for February 1812.]

TO MR. NICHOLSON.

SIR, Edinburgh, December 28, 1811.

THE state of my health has not allowed me to send you an earlier account of the further experiments on the nature of muriatic and oximuriatic acids, which I announced in my last communication. I now beg leave to submit them to the attention of your readers.

I have already observed (Journal, vol. xxviii, p. 139,) that there are two modes of investigation, by which the question at present under discussion with regard to the nature of the relation between muriatic and oximuriatic acids may be determined. Either it may be shown, that oximuriatic acid does or does not contain oxigen; or it may be proved, that muriatic acid gas does or does not contain water.

If it be proved, that oximuriatic acid contains oxigen, then it must be regarded as a compound of that element with muriatic acid, and the discussion is at once terminated.

The other mode, though less direct, is equally conclusive. In the experiment of the mutual action of oximuriatic gas and

hidrogen gas, muriatic acid gas is the sole product. Mr. Davy regards it as a compound formed by their union; and, if it can be shown to be the real acid free from water, or any other ponderable matter, this is the conclusion, which appears necessarily to follow. But, if muriatic acid gas contain water, the conclusion is inadmissible; the origin of this water must be accounted for; and there is no other mode of doing this, but by the established theory, that oximuriatic acid is a compound of muriatic acid and oxigen; and that, in its action on hidrogen gas, its oxigen combines with the hidrogen, forming water, which the muriatic acid, its other element, holds combined with it in the gaseous form. The proof therefore of the existence of water in muriatic acid gas is a conclusive proof of the truth of that theory, and at the same time a demonstration of the falsity of the opposite hypothesis. My former experiments were designed to gain proof of the existence of oxigen in oximuriatic acid: those which I have now to state were undertaken with the view of obtaining evidence of the existence of water in muriatic acid gas.

The difficulty is to find in this mode of investigation an experiment, which shall be conclusive. Such is the facility with which both hypotheses may be adapted to the phenomena, that there is scarcely a case of chemical action exerted either by muriatic or oximuriatic acid, in which an explanation may not be given in conformity to the one as well as to the other. And although the explanations afforded by the common system are less complicated than those of the other, and are more conformable to analogy from similar cases of chemical action exerted by other acids, yet still, since a possible explanation may be given by the latter, the question remains so far undecided.

This observation applies to the experiments, from which it was inferred, that water exists in muriatic acid gas; though at first view they appear to prove it, the proof must be admitted to be doubtful, as they admit of explanation on the opposite opinion. Thus the proof from the agency of water in facilitating the expulsion of muriatic acid gas from dry muriates is ambiguous, as the water may be supposed to operate either by its affinity to the acid, or by affording hidrogen to form it.

The production of hidrogen, when metals are acted on by muriatic acid gas, is a proof of equal ambiguity; since it may be supposed to be derived either from the decomposition of the acid, or of water existing in it. Even apparently the most conclusive of all these facts,—the production of water, when muriatic acid gas is acted on by substances with which acids in general combine, as for example the metallic oxides, admits of this double explanation. The acid is absorbed; and it might be inferred, that it combines with the metallic oxide, while the water which appears is deposited from the gas, in which it had previously existed in a state of combination. But this conclusion, though conformable to the most extensive and strict analogy, is avoided on the opposite hypothesis of muriatic acid gas being a compound of oximuriatic acid and hidrogen, by the supposition, that the acid is decomposed, that its hidrogen combines with the oxigen of the metallic oxide, and forms this water, while the metal itself combines with the oximuriatic acid.

If we can procure however a substance not oxidated, and yet capable of combining with muriatic acid, this source of ambiguity is avoided, and the experiment may be rendered conclusive. There is only one such substance—ammonia. No oxigen can be detected in its composition, and Mr. Davy himself admits, that it combines directly with muriatic acid, and does not decompose it. It cannot therefore cause any formation of water. Neither can it be supposed to afford water; for, when dried by exposure to substances having a strong affinity to water, it retains no sensible portion; nor is any discovered to have existed in it, when it is decomposed. Its combination with muriatic acid gas is thus calculated, I conceive, to afford what is so desirable, yet difficult to attain in the present question, an *experimentum crucis*. If, on combining dry ammoniacal gas with muriatic acid gas, no water is obtained, the result is so far in conformity to Mr. Davy's theory; and it may be concluded, that the water obtained in other combinations of muriatic acid gas has not pre-existed in it, but is directly formed. If, on the contrary, water is obtained; as it does not pre-exist in the ammoniacal gas, and as there is no such mode of accounting for its production as in those cases where oxigen is present, the water must be inferred to have existed in the muriatic acid

gas, and the truth of the common opinion is of course established. To ascertain the fact the following experiments were made.

Ammoniacal gas was dried carefully by exposing it over dry quicksilver to the action of quicklime. Muriatic acid gas received over dry quicksilver was combined with it, to neutralization: or rather leaving a very slight excess of alkali, to guard more effectually against any excess of acid, which might communicate to the product a slight degree of deliquescence. Thirty cubic inches of muriatic acid gas, and thirty-two cubic inches of ammoniacal gas, were employed. The white spongy salt was collected from the sides of the jar. It gave indications of humidity: for, although the surface of it appeared loose and spongy, it could not be entirely detached from the glass, but adhered slightly to it; in removing it by a knife, it spread a little over the surface, as any substance very slightly moist and clammy would do; and, when pressed together by a knife, its parts adhered slightly. It was put immediately into a small glass retort with a long neck, which was connected with a small receiver having two tubulatures, into one of which the tube of the retort was fitted by grinding, and into the other a long straight tube of narrow diameter, open at both extremities, was inserted. The retort being placed in sand, heat was applied by a lamp. In a short time a thin film of moisture condensed in the neck of the retort, which increased and collected into small globules, which accumulated, and trickled down: the heat being applied gently, that the salt itself might not be volatilized, there was no sensible condensation of humidity on the sides of the receiver, or in the tube inserted into it. When the further condensation of moisture appeared to have ceased, the lamp was withdrawn, the retort was cut, and the residual salt removed; a little of it, which had been volatilized, and formed a thin film on the upper part of the retort, being collected, and added to the other portion. The salt had lost in weight 1.3 gr.—a loss obviously to be ascribed to the expulsion of water, and the quantity condensed in the neck of the retort appeared fully equal to this. This is the smallest portion too, that was obtained in frequent repetitions of the experiment, and in some of these the quantity was equal to 1.5 gr.; a difference depending probably on the temperature applied.

100 cubic inches of muriatic acid gas weighing 39 grains, 30 cubic inches weigh 11.7 gr.; and this affording 1.3 of water gives the proportion of $\frac{1}{9}$ of its weight.

It could not be presumed however, that in this experiment the whole water of the compound salt was disengaged. In every case of the combination of an acid with any base, part of the water of the acid enters with it into the combination, at least when the product is a soluble salt, and is not easily entirely abstracted. There is no reason to suppose, that this should not be the case in the combination of muriatic acid and ammonia; and there must be even a greater difficulty in expelling this water from an ammoniacal salt by heat, than from other salts, on account of its volatility. There is another difficulty in the present case; we cannot introduce the affinity of any other substance to the acid, which, combining with it, might allow a portion of the water to be disengaged; for we can employ no substance with this view, but one which is oxidated, and which would therefore introduce a source of ambiguity, as it might be supposed, on Mr. Davy's hypothesis, to *form* water by its action on the acid itself.

The most direct method of discovering any further portion of water in the salt, free from this ambiguity, appeared to be to expose it to a red heat in mixture with charcoal; for, although the whole quantity could not be expected to be abstracted even by this mode, yet a portion might be expelled at so high a temperature, and the charcoal might also by its strong attractions to the elements of water abstract a portion, which would be indicated by the production of its compounds with these elements. The following experiment was accordingly made.

Charcoal in powder was exposed in a clear iron tube, the open extremity of which terminated in quicksilver, to a heat gradually raised to a very high degree of intensity; and this was kept up until the production of elastic fluid ceased. The charcoal was allowed to cool in the tube without the admission of air, and, when nearly cold, the salt remaining in the former experiment was mixed with about an equal weight of it. This was put into a Wedgwood's earthenware tube; the tube was nearly filled with the same charcoal, and was placed across a

small furnace, and surrounded with burning charcoal, so that the middle of it was raised to a red heat. A sufficient heat was thus communicated to the closed end of the tube to volatilize the ammoniacal salt, and cause it to pass through the ignited charcoal; to the other extremity a bent glass tube was adapted, terminating under an inverted jar filled with mercury in the mercurial trough. Elastic fluid began to come over; this was accompanied with a condensation of water in the curved glass tube; the gas itself very soon came over opaque, and humidity appeared on the sides of the jar, and the surface of the mercury within it. When two jars, containing about 14 cubic inches, had been filled, the gas which came over had become transparent; from 15 to 20 cubic inches were produced. Portions of this elastic fluid exposed to limewater caused a milkiness in it, with diminution of volume; the residual gas, after slight agitation with water, burned with the faint yellow flame of hidrogen, and, after its combustion, rendered limewater slightly milky. The charcoal in the tube being agitated with water, the liquor filtered from it was limpid, it had a strong saline taste, and on the addition of potash or lime exhaled a strong ammoniacal smell.

The rationale of this experiment is sufficiently obvious. From the temperature being much higher than in the preceding experiment, an additional quantity of water was expelled from the muriate, its separation being aided by the mechanical effect of the charcoal, which, while it impeded the sublimation of the salt through the whole length of the tube, would allow the more highly elastic watery vapour to pass. At the same time a portion of this water suffered decomposition, producing, by combination of its elements with the charcoal, carbonic acid, and carburetted or oxicarburetted hidrogen gas. The quantity of carbonic acid was from 1 to 1.3 cubic inch, estimated from the diminution of volume.

In both these experiments then, or rather in these two stages of the same experiment, the presence of water in the compound formed by the union of muriatic acid gas with dry ammoniacal gas is demonstrated. Its disengagement from the salt in the first stage of the experiment was not in the least ambiguous, and the quantity was even considerable in relation

to the quantity of acid gas employed, being equal to a ninth of its weight. And, as has been already remarked, this cannot be supposed to be the whole. Had there been no sensible production of water, the presence of any in the gasses combined could not have been inferred; and it could not therefore have been inferred with certainty, that any existed in the concrete salt. But since water was produced, and its existence in one or both of the elements of the salt is thus demonstrated, it is further certain, if we can rely on any conclusion from the most strict and extensive analogy, that the whole quantity could not be expelled by the heat applied.

In the second stage of the experiment, the disengagement of a further portion of water was abundantly evident, though from the nature of the experiment, it was difficult to ascertain its quantity with the same precision. Judging from the appearance of the condensed moisture in the curved glass tube, and in the jars, the quantity was nearly equal to that condensed in the first stage of the experiment; and to this is to be added the quantity decomposed by the ignited charcoal, which formed the carbonic acid and carburetted hidrogen. Adding these, and taking the average of the experiments, I would not hesitate in estimating it equal to the quantity, which appeared in the first stage of the experiment. This, supposing it to be derived from the muriatic acid gas (and, as has been shown, this can be the only origin assigned to it,) gives 2.6 of water in 30 cubic inches, or 11.7 grains of the acid equal to $\frac{2}{9}$ of its weight. The quantity of carbonic acid, (and this could be estimated with accuracy,) taking it at one cubic inch, contains as much oxigen as is contained in ·5 gr. of water, and this of itself added to the quantity obtained in the first stage of the experiment makes the water amount to $\frac{1}{8}$ nearly of the weight of the acid; with the addition therefore of the moisture visibly condensed in the tube and jars, the quantity cannot be less than between a fourth and fifth of its weight.

It may be remarked too, that though the quantity obtained in this stage of the experiment may not admit of being estimated with perfect precision, there is no source of fallacy with regard to its production. The charcoal had ceased to give out gas at a heat of much higher intensity than that to which it was

afterward exposed in mixture with the muriate; the water therefore, or the elastic fluid obtained, could not have been derived from it; and indeed this water appeared at the very commencement of the experiment, when the heat was scarcely equal to that of ignition. If the charcoal afforded any gas too, it could only be a portion of the carburetted hidrogen, and on the quantity of this produced no stress has been laid in drawing the conclusion from the experiment. And it is to be repeated, that the existence of water in the muriatic acid gas to the extent at least of $\frac{1}{5}$ of its weight is demonstrated in the first stage of the experiment, and that, from what must remain in the compound salt, the quantity must be greater than this.

Not only is the presence of water demonstrated by this experiment, but the quantity is nearly the same as that indicated by the action of other substances, which are supposed by Mr Davy to form it by affording oxigen. Thus Gay-Lussac and Thenard have inferred from the action of oxide of silver or of lead on muriatic acid gas, that it contains very nearly a fourth of its weight of water; and the quantity, which may be fairly inferred from the preceding experiments, is nearly the same.* Although it is not necessary, that the quantity should be proved to amount to this, to refute Mr. Davy's hypothesis, and establish the common theory, yet it is satisfactory to have this coincidence. And it must be further admitted as a proof, that the oxigen of these oxides has no share in the production of this water: for it is obvious, that, were the water, which is deposited when muriatic acid gas acts on metallic oxides, on the fixed alkalis, or the earths, formed by the oxigen of these substances, and not derived from the gas as previously existing in it, there can be no production of it in the mutual action of muriatic acid gas and ammonia, as ammonia cannot afford oxigen. Since it is produced in that action it must be derived

* The estimate by Gay-Lussac of the quantity of water in muriatic acid gas being equal to 1-4th of its weight is inferred from experiments, in which the product of the combination of the acid with the base is insoluble, and appears to have no affinity to water, as muriate of silver or of lead. It may be inferred, therefore, to retain little or none of the water of the acid, and hence the production of water to the amount of 1-5th or even 1-6th of the weight of the acid, in an experiment where the product must retain a portion of the water combined with it, is a near coincidence.

from the muriatic acid gas, and the same origin must be assigned to it in the other combinations of this acid.

This experiment then has the advantage of being conclusive on the subject of the present discussion; the state of the fact only requires to be ascertained, and with due precaution this is not difficult of attainment. There is at least no mode of accounting for the production of water, but by assumptions so gratuitous and unfounded, as to be equal to the refutation of the theory. Such is the only assumption that can be made—that the water may be derived from the ammoniacal gas, and not from the muriatic acid gas. When ammoniacal gas is dried by potash or lime, no water can be discovered in it by any test, nor is there any fact which affords a presumption that it contains water; the supposition therefore that it does would be purely gratuitous, obviously advanced to support an hypothesis. But further, dry ammoniacal gas is resolved by the action of electricity into hidrogen and nitrogen gasses; there is no deposition of moisture, and there is no intermixture of oxigen, as there must be were the water decomposed by the electricity. If the water obtained in the preceding experiments were supposed to be derived from the ammonia, it must therefore be maintained without any proof; it is contrary to all probability, that these gasses, which have scarcely any sensible attraction to water, should be capable of holding in solution the large portion indicated by the experiment. And if recourse be had to the hypothesis of unknown quantities of water in gasses, and if all these assumptions are to be made without any proof, are there not much stronger reasons for admitting its existence in muriatic acid gas, the affinity of which to water is so strong? It is obvious however, that were assumptions so numerous and gratuitous to be admitted in defence of an hypothesis, no experiment in chemistry could be rendered conclusive. That the water obtained in this experiment can have no such origin is further apparent from comparing the quantity of it with the quantity of ammonia. The specific gravity of ammoniacal gas is to that of muriatic acid gas as 60 to 124, or it is less than one half. In combining them about equal volumes were employed. Since the quantity of water obtained was equal to at least $\frac{1}{5}$ of the weight of the acid gas, it is equal of course to

$\frac{2}{3}$ of the other. If that water then were supposed to be derived from the ammoniacal gas, and on Mr. Davy's hypothesis it would be necessary to suppose the whole of it derived from this source, we must suppose, that, after being dried, this gas contains nearly half its weight of water. Yet no portion of this can be discovered in it, nor even detected when it is resolved by decomposition into its elements, hidrogen and nitrogen gasses. To add any illustration to this would be superfluous.

The statement of some additional experiments on this subject, and of a few experiments likewise on some of the compounds, as Mr. Davy regards them, of the oximuriatic principle with metallic bases, I must, from the length of this, reserve for another communication. I am, with much respect, your most obedient servant,

JOHN MURRAY.

P. S. In stating in my last letter, that the result I had observed, of dry oximuriatic acid gas not acting on carbonic oxide gas, was confirmed by the very same result having been obtained by Gay-Lussac and Thenard, I ought to have added, that it had also been obtained by Mr. Davy. In his account of "a combination of oximuriatic gas and oxigen gas," he states, among other properties of oximuriatic gas prepared in its pure state, that "it does not act on nitrous gas, or muriatic acid, or carbonic oxide, or sulphureous gasses, when they have been carefully dried."* That Mr. Davy does not state this on the authority of others is evident, not only from the manner in which the sentence is expressed, but also from this, that he is giving an account of the properties of this gas in its state of purity, in which state there was no certainty of its having been obtained in former experiments, as chemists were not aware, that it might have an intermixture of oxigen, by which its properties and chemical agencies are materially modified. He gives this as a property of the *pure* gas, and of course he would not have done so without having ascertained it.

* Phil. Trans. for 1811, p. 156; or Journal, vol. xxix, p. 269.

SELECTED REVIEWS.

Relation Historique et Chirurgicale de l'Expédition de l'Armée d'Orient, en Egypte et en Syria. Par D. J. Larrey, *Docteur de l'Ecole Spéciale de Médecine de Paris, Chirurgien en Chef de l'Armée d'Orient, &c. A Paris, Chez Demonvelle et Sœurs.* 8vo. 1803. pp. 480.

[From the London Medical Review for July 1811.]

THE length of time that has elapsed since the publication of the volume before us, and the extracts that have already been presented to the public, do not discourage us from laying an analysis of it before our readers, when we consider the high professional, and high public importance of some of the facts which it contains, and the small number of copies of the work that have yet reached this country.

As soon as the expedition into Egypt was decreed by the Directory, M. Larrey was appointed surgeon in chief, and M. Desgenettes (who in his commission is styled Médecin en Chef de l'Armée d'Angleterre) physician in chief to the army of the East, as the French were pleased to entitle it, which was to sail under the command of Bonaparte. To these two gentlemen was confided the whole direction of the medical arrangement of the expedition. They received from government a discretionary authority, not only to provide themselves with whatever they might desire for the completest furnishing of their respective departments, but to make up according to their own judgment, the medical and surgical establishment of that army. They applied accordingly to the professors of the schools of medicine at Montpellier and at Thoulouse, specifying the number and qualifications of the assistants they required, and desiring them to furnish them. In consequence of this application, M. Larrey procured one hundred and eight surgeons, exclusive of those regularly attached to the regiments. M. Desgenettes found more difficulty in supplying himself with a sufficient number of physicians. He applied to the school of Montpellier for six, expecting the complement to be made up by the army physicians; but the greater part of

these disappointing him, he was obliged to make a second application at Montpellier for as many more. The number ultimately under his orders he does not specify, but they were not fewer than seventeen, since he accounts for that number in the course of his work. Such was the medical establishment of the army of the East, amounting, according to Larrey and Desgenettes, to 30,000 men.

On the arrival of the army in Egypt, and the capture of Alexandria, having found an hospital in that town, he arranged his medical staff in six divisions, one being attached to each of the five brigades of the army, and the sixth, which he calls a corps de reserve of surgeons, being stationed under his own immediate direction, at headquarters. And afterwards, at Grand Cairo, and in every place of which the French took possession, hospitals with a proper medical establishment were formed. But besides these fixed hospitals, (*sédentaires*, Larrey calls them,) there were attached to the army camp-hospitals, as they may be called, (*ambulances* is his name for them,) which accompanied it in its marches, and in the field. During engagements they were disposed along the line in the most convenient situations for giving immediate assistance to the wounded; those whose wounds required immediate and capital operations being carried to the *ambulance* of the centre, where Larrey himself was stationed. At the first battle of Aboukir, in which the Turks were defeated, the French had 800 wounded, besides four generals, among whom were Lasnes and Murat.

"The most severe wounds," says M. Larrey, "were brought to the *ambulance* of the centre; I dressed them myself, and performed the necessary operations. Forty amputations were performed immediately, and with wonderful success. The wounded received on that occasion, from the surgeons belonging to the regiments and from the *ambulanos*,* the readiest and most efficacious assistance. *No one had to wait more than a quarter of an hour before his wounds were dressed.*"

At the battle of Alexandria, in which the French were defeated by the army under Sir Ralph Abercrombie, on the 21st of March, they had 1300 wounded; six hundred had been

* Surgeon attached to the staff, and not any particular corps.

wounded in the preceding actions of the 8th and 13th of the same month; eighteen hospitals were filled with them.

"The greater number were dressed, and had the necessary operations performed on the field of battle, or immediately after they were conveyed to the hospitals at Alexandria. All on whom amputation was performed within a few hours, were cured in a short time, and without untoward symptoms."†

No account we believe has been published of the fate of our wounded after the battle of Alexandria; but we know, from certain authority, that though victorious, many of them fared worse than the conquered. Although, in some instances, they received immediate assistance in the field, yet they were removed, soon after they had been dressed, or had undergone operations, to the fleet; and as the number of wounded far exceeded the means of accommodation that had been prepared for them, much misery ensued, and many lives were lost which with due care and assistance, might have been preserved. Orders were sent for several transports to receive as many wounded as they could contain. They were soon crowded, and boats, loaded with wounded, were seen rowing about in Aboukir-bay, for some time before a ship was found that could receive them; and when at length they were on board, there was, in some vessels, a total want of medical aid, as the number of staff-surgeons was not sufficient to supply the ships. It is true that the wounded derived great benefit from the attention of the navy surgeons; but, as some of these gentlemen were obliged to return to their own ships in the evening, many unfortunate men were left during the night without that attendance which their situation required.

We are aware that misery is inseparably connected with war, and we do not mean to censure the individuals who compose the medical establishment of our army. The fault does not lie with them; they are willing and able to do their duty. But where the medical officers are so few, where regiments on actual service have not always their complement of surgeons, how is it possible that the immense number of wounded that are brought to their quarters can be properly attended to? We do

† Page 435.

not know what difference there is in the number of regimental surgeons in the British and French service: but we have strong reason to believe (though we do not speak from positive authority) that the proportion is far greater among the French. And we should be inclined to trace, in the greater attention which the surgeons among our enemies have it in their power from their superior number, to give to their patients, the cause of a prejudice not common among soldiers and sailors in favour of the French surgeons, and which we have known to extend even to officers.

One great disadvantage of the insufficient number of medical officers is apparently that their work cannot be done in time; which in most cases of serious operation, means, that a life which might have been saved is lost. In speaking on one occasion of wounds of the limbs, from musket and cannon balls, which require amputation, M. Larrey observes, that "all wounded in such circumstances, on whom the operation is not performed within a few hours after the accident, die on their way to the hospital, or soon after entering it."* But besides the lives lost from delay, it is to be considered that where the work to be done is altogether beyond a man's power to accomplish, there are very few that will not be thrown into hurry and confusion, and do ill what their time allows them to undertake; very few who will have strength of mind in that distress to select what can be done, and apply their thoughts only to what they have decided to do. What will be the conduct of an ordinary man in such a situation? A wound is brought before him, and the life of the patient requires an immediate operation; but a dangerous operation is at all times formidable, and especially on a field of battle, if a surgeon is not within reach of the counsel and asistance of his brethren. Often from timidity, and oftener wearied out by the heart-rending importunities of the poor fellows who are waiting their turn to be dressed, he will be seduced to hope that no harm will happen from a little delay, and to put off the operation.

The only effectual means which we can imagine of supplying relief to the wounded of an army actually in battle, is by such

* Page 436.

an arrangement as M. Larrey has described under the name of *ambulance*. This plan had been suggested by the celebrated Ranby, sergeant-surgeon to George the second whom he accompanied in his German wars. He had witnessed the miserable insufficiency of the ordinary supply for the emergencies of an engaging army, and describes in the little treatise on gun-shot wounds, suggested by his practice in those wars, the method of which he foresaw the success, but which has never been fully acted upon in our service.

" I could wish to be indulged," says Ranby, " in offering a scheme which might, I think, be put in execution, with all the facility imaginable. It is this: when the army is forming for an engagement, let the surgeons, with their respective mates of the three or four regiments that are posted next each other, collect themselves into a body, (the same method being observed throughout the line,) and take their station in the rear, according to the command of the general. Here let the wounded be put under immediate care and management. By this means they will be enabled mutually to assist each other, and to perform their duty both with more exactness and despatch. Now let any person of common humanity reflect on the consequences naturally flowing from this manner of treating the wounded on the spot; having first formed an idea of poor creatures being actually conveying, though with all conceivable tenderness, from place to place, under the extreme misery of large, lacerated wounds, bleeding arteries, or fractured limbs; and this abrupt, preposterous removal attended with perhaps the most fatal symptoms, of which there was not perhaps at first the least appearance or apprehension. I am apt to think that such a one will not hesitate a moment in preferring this my scheme to that scene of terrible distress, which I look upon as one of the most moving that can be presented to the human eye."*

But to make these camp-hospitals effectual, of course another greater alteration in the present system is requisite: a larger medical establishment. When troops are healthy, and not near an enemy, the present establishment of the regiment is more than sufficient; but in an enemy's country, and in an unhealthy

* Ranby on Gun-shot Wounds, page 33.

climate, it is altogether insufficient; and the medical staff of the army is likewise inadequate to the exigencies of the service. The system of warfare is now more bloody than it ever was. The greater use of the field artillery and of the bayonet in our present battles is attended with a havock, which by adding to the number of the wounded, increases the labour of the surgeons, and renders the augmentation of their number absolutely necessary. This augmentation must be adapted to the occasion. When an expedition is fitted out, in which a severe contest with the enemy may be expected, most especially when it is destined to an unhealthy climate, let government show its care of the welfare of its soldiers by raising a numerous but temporary medical staff, as was done in France. It may be done here as easily as in France, and as well. The emulation which Larrey tells us appeared in their schools for the appointment of that service, might be seen in our own, provided government could engage the confidence of the young men of the profession, and satisfy them that merit would find its own way to distinction; a trust which it could easily inspire, as was done there, by its first and leading appointments; nominating to the head and command of the whole medical establishment, not those whose names were presented by the lists of seniority and service, or obtruded by interest, but those whom their own pre-eminence in genius and knowledge had already marked out in the eyes of the country, as the legitimate claimants of the noblest office with which the country can honour the profession—the superintendance of the health and lives of its defenders.

M. Larrey professes to give an historical as well as a chirurgical account of the Egyptian expedition; he is, however, extremely concise, and very properly so, in speaking of whatever relates to the military operations of the French army. The order of their occcurrence has, in a great measure, fixed the order of the professional contents of this volume, and they chiefly serve as dates in the journal which he kept in his own department. As we wish to lay before our readers whatever appears to us most interesting in point of novelty and importance, we shall insert extracts chiefly from the cases, some of which are valuable on many accounts. The chapters on the plague, and on ophthalmia, though treating of the two most re-

markable complaints in Egypt, and obviously the result of much experience, seem to us to contain very little interesting matter: to these therefore, we shall not call the reader's attention; yet we cannot help observing the singular fact that neither M. Larrey nor M. Desgenettes, who have both written on the plague, should have been acquainted with the good effects of mercury in that disease; and yet M. L. had observed the analogy between the plague and the yellow fever of the West Indies, and could scarcely fail to be acquainted with the latter complaint.*

The French lost a number of their wounded from tetanus; in some it proved fatal in three or four days, in a few the disease lasted longer. M. L. observed in his patients, "if not an abhorrence of fluids, at least a considerable repugnance to them;" in consequence of which it was difficult to administer internal remedies. In the case of a surgeon who died of that dreadful affection, in consequence of a wound of the face, he endeavoured to introduce into the œsophagus a tube of elastic gum; but he found it impracticable, as it occasioned convulsions and suffocation. On opening the bodies of patients that had died of locked jaw, he found "the pharynx and œsophagus in a state of great constriction, their internal membrane red, inflamed, and lined with a viscid and reddish substance."†

Frictions with oil, strongly recommended by some authors, were inefficacious. Mercurial frictions seemed to M. L. to aggravate the symptoms. He tried, but without any success, an application of tobacco leaves, to the wounds of patients affected with tetanus. Moxa and the actual cautery were equally unsuccessful. He was not more fortunate from the use of blisters, except when the tetanus was preceded by the cessation of suppuration from the surface of a wound; in such cases he applied blisters with advantage, and their good effects were attended with a renewal of the suppuration. The favourable result of a case of tetanus in an officer, who was shot in the foot, and whose leg was amputated, after opium, tobacco, camphor and

* The same analogy has been observed by Drs. M'Gregor and Chisholm.
† Page 58.

other remedies had been used in vain, led M. Larrey to recommend the same practice in other cases; and his success supports the opinion which he advances, that in all cases of tetanus from wounds of the limbs, it would be adviseable to amputate as soon as the symptoms appear. When the officer whose case we allude to submitted to amputation, his life was despaired of; he himself wished for the operation, and it was performed as a last resource, after every other remedy had been tried in vain. The symptoms ceased almost immediately on removing the limb in which the wound was situated; even where the case in the end terminated fatally, the operation relieved the symptoms very considerably. It should seem however, from one case, that a division of the soft parts in the vicinity of the wound, was sufficient to relieve the symptoms of tetanus, at least in the beginning. The general of division, Destaing, was shot through the middle and back part of the right arm, at the battle of Alexandria; the ball had penetrated through the muscles and had injured the nerves of the arm; convulsive motions of the limb, considerable anguish, and some difficulty of breathing took place soon after the wound was inflicted. At the end of eight days, during which the patient had suffered considerably from pain, febrile symptoms, and loss of appetite, M. Larrey was called in. He wished immediately to lay open the wound along the course of the ball, but to this the patient refused to submit, and emollient applications were employed. However, on the following day, symptoms of tetanus having made their appearance, and the patient being in a state of great anxiety, the wound was laid open by dividing the integuments, the cellular membrane and the few muscular fibres which covered the course of the ball; the lower part of the wound was likewise scarified. The operation was painful, but in two hours the patient felt considerably relieved, and at the end of two days the alarming symptoms entirely disappeared, and the patient recovered.*

During the campaign in Egypt, the operation of extirpating the arm at the shoulder joint occurred nineteen times; thirteen of the patients did well, the remaining six died of internal diseases.

* Page 81.

This operation has been performed several times in this country, and by our army surgeons; and the manner of performing it is so generally known, that we deem it unnecessary to say any thing further on that subject.

We do not know that the operation of extirpating the thigh at the hip joint was ever performed by any surgeon in this country except by Dr. Kerr of Northampton.† M. Larrey has performed it three times, twice in Egypt and once while he was surgeon to the army on the Rhine. His three patients died; but when it is considered that one of these survived the operation a week, at the end of which he was carried off by the plague, and that the others died after being conveyed in a very uneasy manner during a precipitate march of the army, we are not warranted, especially after the successful case that is already upon record,‡ in declaring the operation to be impracticable, and we should think very ill of a surgeon who, under the circumstances in which it is called for, should not give his patient the remaining chance of life.

The excision of joints is one of the most important improvements of modern surgery, and for the manner in which M. Larrey has employed that practice in the treatment of gun-shot wounds, he deserves the greatest credit. He has been able to save limbs, which according to the rules of surgery, should have been taken off; and when the importance of saving an upper extremity, for which no substitute can be supplied, is considered, the value of his practice may be appreciated. When the arm is fractured near its upper extremity by a musket ball, it is considered by most surgeons necessary to amputate the limb; and in such cases the operation used invariably to be performed; but says M. Larrey,

"I have had the good fortune, on ten different occasions, of superseding the necessity of the operation, by *the complete and immediate extraction of the head of the humerus and of the splinters.* I perform the operation in the following manner. I make an incision in the centre of the deltoid muscle, and parallel to its fibres, carrying the incision as low down as possible.

† Vide Dr. Duncan's Med. Commentaries, vol. vi. p. 337.
‡ Sabatier Medicine Operat. tom. iii. p. 330.

I get the edges of the wound drawn asunder, in order to lay bare the articulation, of which the capsule is generally opened by the first incision, and by means of a probe pointed bistoury I detach with the greatest ease from their insertions the tendons of the supra and infra spinati, of the teres minor, of the infra scapular, and of the long head of the biceps; then I disengage the head of the humerus, and remove it through the wound in the deltoid by means of my fingers, or of an elevator. I bring the humerus up to the shoulder, and fix it in a proper position by means of a sling and a bandage. Such is the operation which I performed on ten patients, in extirpating the head of the humerus; one of these died of the hospital fever, two of the scurvy, at Alexandria, and the fourth, after he was cured, died of the plague in our return to Syria. The rest returned to France in good health. The arm became anchylosed to the shoulder in some, and an artificial joint allowing of motion was formed in others."*

Our limits forbid our giving the details of the cases in which this operation was performed, some of which are highly interesting. In the case of a drummer, seventeen years of age, whose shoulder was struck by a four-pounder, the humeral extremity of the clavicle, the acromion and the head of the humerus were so shattered that it was necessary to remove all these parts; this young man recovered completely, and his arm became anchylosed to his shoulder.

Perhaps the most important part of M. Larrey's work, is that on amputation in consequence of gun-shot wounds; this memoir as it is called, was written as an inaugural dissertation for the degree of doctor of medicine, and its principal object is to recommend immediate amputation in all cases of gun-shot wounds which require that operation. In the year 1754, the academy of surgery offered a prize for the best dissertation on gun-shot wounds, and on the most proper time for performing amputation, when that operation became necessary in consequence of such wounds. After considerable debates, the prize was adjudged to the dissertation of M. Faure. The object of his dissertation is to recommend a delay of the operation, and

* Page315.

M. Larrey observes, that the principles of that author have been since adopted by almost all practitioners. The practice of M. Faure is followed by the most eminent surgeons of the present day in this country; it is recommended by Mr. John Hunter, in his treatise on gun-shot wounds; the support however which it derives from the notice of Mr. Hunter, arises more from the authority of his name than from the strength of his arguments in its favour. That part of Mr. Hunter's works is in our opinion, the weakest of all his practical writings. The propriety of immediate amputation, when its necessity is admitted to be sooner or later unavoidable, is enforced by writers whose experience gives considerable importance to their opinions. Suffice it to name Le Dran, Ranby, Kirkland, and Larrey. It would be presumptuous in us to attempt to decide a question so important and so difficult; but after considering the subject with attention, we believe it may be safely affirmed that the propriety of delaying amputation till the first inflammation is over, is far from being so firmly established as Mr. Hunter thought, and as many surgeons still maintain. The work of M. Larrey contains a collection of facts more conclusive in favour of immediate amputation than any that we have met with in any other author; and he reasons from the only experience to be relied on, viz. an experience of the comparative merits of both methods. It would exceed our limits were we to attempt to lay before the reader an analysis of that part of the work; we must content ourselves with having given our opinion of its importance, and refer to the work itself for the details.

Among the most singular wounds that occurred in M. Larrey's practice is one of a soldier, whose epiglottis was shot off at the battle of Alexandria. " The ball entered at the angle of the jaw, crossed the mouth obliquely, and went out at the opposite side, (*et sortit à la region jugulaire du coté opposé.*) The base of the tongue was grazed and the epiglottis shot away; the patient spat it out some time after, and showed it to the surgeon who first dressed him. The patient suffered little, but he could not produce articulate sounds, and his voice, which was hoarse and feeble, was heard with difficulty." Every attempt which he made to drink was attended with a convulsive cough, and with a sense of suffocation and vomiting; he

remained in that deplorable condition for four days; on the fifth day M. Larrey was called in; he introduced into the œsophagus a flexible tube, through which the patient was fed for six weeks; at the end of that time he was able to eat without the tube thick panado and thickened rice, which he formed into little balls, and swallowed with tolerable ease. His powers of speech and deglutition improved very much afterwards, in consequence as M. Larrey imagines, of the office of the epiglottis being supplied by an enlargement of the arytenöid cartilages, and by an expansion of that part of the base of the tongue which lies next to the glottis.*

Another very singular circumstance observed by M. Larrey in Egypt, was an atrophy of the testicles, from which a number of the French soldiers suffered.

"Both testicles, but more generally one, loses in part its sensibility, becomes soft, diminishes in size gradually, and appears to dry up. The patient is not sensible of his loss till the testicles are very much reduced in size, and then they are found lying at the ring, of the shape and size of a French bean. All those to whom this accident has happened, have lost all venereal desires, and become impotent, and the whole system has been weakened in consequence of that loss."†

We have already exceeded our proper limits, and yet our analysis is very imperfect; but we trust the reader will be able from these extracts to form some opinion of the merits of this volume. It is the work of one of the first military surgeons of the present day, raised to high rank in his profession at the beginning of a revolution, a time when merit never fails to be discovered and rewarded. To know that he has been employed in the principal campaigns of the French armies, is sufficient to give an idea of the extent of his experience. On his return to France from Egypt, he was appointed surgeon to the consular guard, and we understand that he is now body surgeon to Bonaparte. We strongly recommend this volume to the perusal of all surgeons, but particularly to army surgeons: it is to be regretted however, that copies of it cannot be readily procured in this country, and that no translation of it has yet been published.

* Page 290. † Page 216.

The Anatomy and Surgical Treatment of Inguinal and Congenital Hernia. By Astley Cooper, F.R.S. &c. &c. Illustrated by plates. London, 1804. Large folio. 2*l.* 2*s.*

[From the Edinburgh Medical and Surgical Journal for 1806.]

This work is already so well known that some of our readers may think it necessary at this time to take notice of it; but the merits of the author, and the importance of the subject, induce us to give a short analysis, and to make a few cursory remarks on its contents.

The object of Mr. Cooper, in the present volume, is to give the anatomical description and surgical treatment of inguinal and congenital hernia, leaving the consideration of femoral, and the other species, to a future publication.

There is no disease where minute anatomical dissection has been more successfully employed than in the pathology and treatment of hernia; for as this complaint arises more from an alteration in the relative situation of organs than from any morbid change of structure, we must be well acquainted with the natural appearances in order to discriminate the effects of disease, or be able to point out the best means of affording relief.

Although Albinus seems to have been well acquainted with this part of anatomy, yet it is to Gimbernat, a celebrated Spanish surgeon, that we are indebted for the first accurate and detailed description of the *abdominal*, and still more particularly of the *crural* rings.

Mr. Cooper, besides availing himself of the description these anatomists have given, advances a step further, and describes some of the more minute parts of the anatomy of the abdominal ring with much precision and ingenuity. Neither Mr. Cooper, however, nor, as far as we know, any other anatomist, has paid sufficient attention to a description of the bones of this part of the pelvis, when describing the soft parts; for we have long believed that an accurate knowledge of them will lead to a simple description, and give a clear notion of the soft parts with which they are connected. If we examine these bones, we find a ridge extending from the inferior part of the

venter of the ilium, which forms the lateral parts of the brim of the pelvis, and the line of division between the pelvis and abdomen, called *linea innominata;* and there is also another line or ridge, which begins at the rough crest or angle of the os pubis, and extends along the upper and inner edge of the bone to meet the former. The junction of the two ridges, in some subjects, is distinctly marked; but, in most bones, a small portion of the edge is rounded at that part where the femoral vessels pass out of the pelvis. Although these two ridges have been described long ago by anatomists, yet they do not seem to have explained their use, or considered them in the description of the soft parts. It will be found, however, that the pubic portion is intended for the insertion of the inflected part of the ligament of Poupart, called Gimbernat's ligament, and the iliac portion for the attachment of the aponeurosis of the iliac, psoic, and abdominal muscles. This knowledge of the bones, and of the different parts of them into which the aponeurotic and tendinous fibres are inserted, enables us to form a clear and distinct notion of the relative situation of parts, and to be aware of the rashness with which particular names have been given to continuations or parts of one fascia, or of the same aponeurotic expansion.

The oblique passage of the spermatic cord through the abdominal parietes was well known to and elegantly delineated by Albinus. Nevertheless, no author, except Mr. Cooper, has considered it with that attention which it seems to merit. After having minutely described this beautiful piece of mechanism, he points out, with a good deal of ingenuity, the necessity of taking it into consideration, in order to explain particular symptoms and varieties of inguinal hernia, to accomplish the reduction of hernia, and also in the application of trusses.

There is one part of the anatomy of which our author has not taken notice. It is the difference of the relative situation of the ligament of Poupart in the male and female. In Mr. Cooper's description no other difference is mentioned, than that the abdominal ring serves for the passage of the spermatic cord in the male, and of the round ligament in the female. Dr. Monro, jun. has observed a remarkable difference in the

formation of these parts in the two sexes, and he has described it accurately, and delineated it in his work on femoral hernia. It may be sufficient here to mention, that this difference accounts very satisfactorily for the greater frequency of femoral hernia in the female than in the male, and for the rare occurrence of inguinal hernia in the female.

The next part of Mr. Cooper's work treats of the pathology of hernia. He describes the manner in which the sac is formed, its size, mode of descent, coverings of fascia, cellular substance, and, as he calls it, cremaster muscle. He also takes particular notice of the situation of the spermatic cord, and mentions two varieties, in one of which, instead of the hernial sac passing on the outside of these vessels, it was found on their inner or pubic side. In the other instance, the vas deferens was found on the one side, and the rest of the cord on the other side of the sac.

The next chapter contains the diagnosis and the enumeration of those causes which most commonly produce hernia.

The subject of the fifth chapter is one of importance, and it is full of good practical information relative to the mode of action, application, and management of trusses.

"The object in applying a truss (says our author) is to close the mouth of the hernial sac and destroy its communication with the abdomen; and this object can never be perfectly fulfilled by any truss which is applied in the usual manner upon the abdomnial ring, and extending from it upon the os pubis. In this case the cure must be incomplete, because a considerable portion of the hernial sac remains uncompressed towards the abdomen, which portion is that situated between the abdominal ring and the opening of the sac into the cavity of the belly." He adds, "The proper method of completely obliterating the mouth of the hernial sac is to apply the truss, not upon the abdominal ring, but upon the part at which the spermatic cord, and with it the hernia, first quit the abdomen; for in this way only can a descent of the hernia be prevented entirely, and a cure by pressure, if practicable, can be performed." "Therefore when a hernia has been returned into the abdomen by the surgeon, he should lay his fingers obliquely above and without the ring, and direct his patient to cough, and the furthest part

from the ring towards the spine of the ilium, where the hernial sac is felt to protrude, is the point which should be noted for the application of the pad of the truss, and the instrument made accordingly."

These remarks are no less ingenious than important. There are two circumstances, however, which appear to us to be of some moment, although they have not been taken notice of by our author. They regard the strength of the spring, and the form and size of the pad. It will be found that a person who has worn a truss for many years is not able to give up using it, chiefly on account of the strength of the abdominal muscles being diminished at that part where the pad rests. The pressure of the pad seems to produce an absorption of the abdominal parietes. As this effect will always be in proportion to the degree of pressure or strength of the spring, it is of importance never to employ in any case a spring stronger than what is necessary to keep up the bowels; and it is useful for a person labouring under hernia to have two trusses of different strengths, one for ordinary purposes, and the other when riding, or when taking any violent exercise. Our author advises that the pad be made of a conical form, the apex of which should rest on the internal ring or mouth of the sac. However useful this may be as a general rule, it ought to be known that there are many exceptions; and, with due deference to Mr. Cooper's opinion, we beg leave to mention, that we have found such a variety, not only in the size of the rupture, but also in the situation and size of the opening through which it passes, that we apprehend it will be necessary to vary the form and bulk of the pad according to every individual case. If the pad is too large and flat, it will prevent the bowels from passing through the external ring, but it will allow them to pass through the internal ring, and enter the inguinal canal. On the other hand, if the pad is so small as to press into the mouth of the sac, and plug it up, there is no longer any chance of a permanent cure. The bowels may be prevented from entering the sac, but the pad will act as a dilator or bougie, keep the mouth of the sac continually open, and even increase its diameter. It therefore appears to us, that, in every case of reducible hernia,

a pad ought to be made according to the size and situation of the ring; that it be of such bulk and form as to make a pressure on the *internal* abdominal ring, along the *inguinal* canal, and on the *external* ring.

IRREDUCIBLE HERNIA.

"The following are the cases which seem to bring the disease into this state.

1. When the protruded parts are suffered to remain long down, they increase so much in size as to be incapable of reduction.

2. When membranous bands form across the sac, entangle its contents, and prevent its free motion.

3. When the protruded parts become closely united by an adhesion to the side of the sac sufficiently firm to render them immovable."

The most efficacious means recommended by our author to accomplish the reduction of a hernia in this state, is to wear a bag truss of such a form as to keep a steady and uniform pressure on the scrotum. The pressure produces a gradual absorption of the adipose matter of the protruded bowels, and thus, after some days confinement, the tumor becomes very much diminished, and at last may be returned. The application of ice occasionally procures the return of a hernia which appears irreducible. Mr. Cooper thinks that the good effects of this remedy are owing to its producing a contraction of the scrotum, which performs the office of a strong and permanent compression of the tumor, and he relates two cases where this practice proved successful.

OF STRANGULATED HERNIA.

"If the tumor be examined after death, a quantity of clear serum will first be found under the skin. The hernial sac contains a portion of bloody serum of a coffee colour. The intestine is of a chocolate brown, with here and there a black spot, which easily breaks down on being touched with the finger. A coat of the coagulable lymph, of the same colour as the intestine, may be peeled from its surface, and adhesions of no great strength are found to extend from the intestine to the sac. At

the particular part where the intestine is strangulated by the constricting membrane, it is either ulcerated through, or readily pulls asunder under slight pressure. If the inflammation has been very extensive, there is a quantity of air in the surrounding cellular membrane."

"On examination after death, in strangulated omental hernia, the omentum is found scarcely changed from its natural appearance; its colour is a little, and but a little, darker than usual. I found it in some cases, even during the operation, extremely offensive to the smell; there is scarcely any fluid in the sac. Though the cavity of the abdomen is inflamed, and the intestines slightly adhering to each other, they never appear to have suffered so much as by intestinal hernia." In old and large herniæ, Mr. Cooper believes that the strangulation is formed most frequently by the external abdominal ring; but, in other cases, it is more commonly seated at the internal ring, the place where the spermatic vessels quit the abdomen. "So if the surgeon, during the operation for hernia, examines accurately into the seat of the strangulation, he will find, except in large herniæ, cutting through the ring is insufficient to relieve the protruded parts, but he must proceed with his knife further up towards the spinous process of the ilium, before he can return the swelling."

"Moreover, though the abdominal ring be dilated with freedom, the hernia will, in many cases, still retain its colour of strangulation, and remain irreducible, as before; but if the sac be traced upwards with the knife, about one inch and a half, midway between the ilium and pubis, the stricture will there be found; and, when this is divided, the intestine recovers its colour, and can be readily returned."

TREATMENT OF STRANGULATED HERNIA.

The first object of our attention, in order to accomplish the reduction of a strangulated hernia, ought to be the position of the body of the patient. The celebrated Winslow, the first anatomist who explained many of the phenomena of the disease from a previous knowledge of the action of the muscles, conceived it to be of importance that the body of the patient be placed in an inclined plane, and that the thighs be bent towards

the trunk of the body. Our author advises the same practice, and remarks that such a posture, by relaxing the fascia of the thigh, relaxes also the aperture through which the hernia passes. There is no doubt but every degree of tightening or relaxation of the femoral fascia will be accompanied by a corresponding change in the abdominal ring; but the motion of flexion, while it relaxes the fascia, relaxes at the same time the abdominal internal iliac and psoi muscles, and it is the change produced by the relaxation of these muscles which facilitates, and ought to be kept in view in the reduction of hernia. To be convinced of the truth of this observation, it is only necessary to observe in the dead body, when in a horizontal posture, the size of the crural ring. By introducing into it the forefinger, a tight cord is readily felt at the upper part; when the thigh is bent upwards and inwards, the cord is relaxed, and the opening is enlarged.

The pressure which is employed on the tumor by the hands of the surgeon should always be directed upwards and outwards along the course of the canal of the cord, and our author advises it to be continued from a quarter to half an hour; besides the mechanical means, he recommends tobacco glisters and cold as the most successful in diminishing the increased action and bulk of the parts. Cold applications have been approved of by the most celebrated surgeons, and they have been particularly recommended by Mr. B. Bell, in the form of ice: when ice cannot be procured, our author uses a mixture of equal parts of nitre and sal-ammonia. To one pound of water in a bladder ten ounces of the mixed salts are added, the bladder tied up, and then laid over the tumor. "If, after four hours, the symptoms become mitigated, and the tumor lessens, this remedy may be persevered in for some time longer; but if they continue with unabated violence, and the tumor resist every attempt to reduction, no further trial should be made of the application."

OPERATION.

There is not a more difficult point in surgery, or one which requires more decision, than to determine the exact period when recourse must be had to the operation. From the dread-

ful consequences of delaying it till the protruded parts mortify, some eminent surgeons have recommended its early performance, whilst others, from the severity and risk which always attend it, advise it to be delayed till every means have failed of procuring a reduction.

Those who urge the early performance of the operation (more particularly Dessault) have founded their opinion on the effects of a similar operation, when there is no hernia, or when the hernia is recent; for, in such cases, no serious consequences are to be apprehended, or any symptoms likely to occur more violent than what takes place after the common operation of hydrocele by incision; on the other hand, many cases have occurred, where, after repeated trials had been made to accomplish a reduction, and the operation about to be performed, the bowel has been suddenly and unexpectedly reduced.

The symptoms which ought to guide us in having recourse to the operation arise from an attack of inflammation in that part of the intestine contained in the hernial sac, and from its spreading into the abdominal cavity. It is in proportion to their violence, and after every fair and probable means has been employed, that we ought to urge the performance of the operation. Mr. Cooper considers pain on pressing the belly, and tension, as the symptoms which point out its immediate necessity. He adds, page 27, "Indeed, there is scarcely any period of the symptoms which should forbid the operation; for even if mortification has actually begun, the operation may be the means of saving life, by promoting the ready separation of gangrenous parts."

Mr. Cooper has explained at great length the different steps of the operation. He directs the incision to be made from the upper part of the abdominal ring to the bottom of the hernial sac. We are warned, however, by Camper against making this extent of incision downwards, for it sometimes happens that the spermatic vessels pass on the anterior part of the sac, and are very apt to be divided.

The sac is to be opened at its inferior part by pinching it up with a pair of forceps, and cutting the elevated portion horizontally with a scalpel.

We witnessed, in one case, a surgeon very much perplexed

when he came to this step of the operation. The sac had a blue transparent colour, and looked very like a piece of strangulated intestine; the surgeon, for some time, conceived that it actually was so, and at last opened it with the utmost terror, when, to his surprise, he found it to be a thin hernial sac, much distended with water. We have seen the same puzzling appearance in the operation for hydrocele by incision, and it is one we may expect to find when either the hernial sac or tunica vaginalis is thin.

In order to divide the stricture, "the surgeon passes his finger into the sac as far as the stricture, and then conveys a probe-pointed bistoury on the fore part of the sac, and, insinuating it within the ring, cuts through it in a direction upwards, opposite to the middle of the sac."

Mr. Cooper thinks there is an advantage from not dividing the *hernial sac* in dilating the ring, as it takes away all danger from wounding the intestine; he also makes some useful remarks on cases where there was more than one stricture.

OF MORTIFICATION OF THE BOWELS.

The next chapter contains many useful practical observations on the mortification of the bowels, and on the artificial anus; also some very interesting and ingenious experiments of Mr. Thomson of Edinburgh, relative to the mode of tying two portions of divided intestine.

It appears from Mr. Thomson's experiments, that if the intestine of an animal be divided transversely, reunited by ligatures, and returned into the abdomen, the animal suffers no inconvenience, and the ligatures are discharged at the anus.

"However (says Mr. Cooper,) as the protruded parts in hernia are so much inflamed as to endanger a speedy separation of the ligatures, and as it appears from my experiments (page 35) that the animal did not suffer from the ligature hanging from the abdomen, I should still prefer performing the operation of uniting the divided intestine in such a manner as to give an opportunity of extracting the ligatures, if any inconveniences arose from their application."

Since Mr. Cooper's work appeared, an essay has been published at Philadelphia by Mr. Smith, which contains a series

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"It appears from the result the not only the intestine may be re nia, abdomen, but that the ligatures ma en with the intestine, as was observed by burgh, and that we need not be under their being discharged into the cavity; for, b the animal economy of which we are ignoran have, in every instance, either been discharged wi or been found loosely attached to the *internal* coat testine.

"It has been said by Messrs. Cooper and Thomson, there is a curious difference in the facility with which a longitudinal and transverse wound unites. But, in all the experiments which I have made, it was found that, with care, the *longitudinal* united as kindly as the transverse, only requiring a little more attention to the diet of the animal, which should be very sparing and liquid, until the wound has had time to heal. It certainly requires more pains to close a longitudinal wound completely than one which is transverse. The longitudinal incision always occasions a diminution in the diameter of the intestinal canal, thereby producing dangerous obstructions. If it be of any considerable extent, probably the surgeon would be justified in cutting out the wounded portion, and treating it as a transverse division. This may be done without much endangering the life of the animal, as appears by two experiments, where three inches of the intestine were removed."*

Mr. Cooper next proceeds to give some account of the mode of dressing, and after-treatment of the patient. He particularly takes notice of the impropriety of giving strong purges, if an evacuation can be procured by more gentle means. He enjoins the patient to be kept in a horizontal posture till

* Vide an Inaugural Essay on wounds of the intestines, for the degree of doctor of medicine, submitted to the examination of the Rev. J. Andrews, D. D Provost (pro tempore,) the Trustees and Medical Professors of the University of Pennsylvania, June 1805. By Thomas Smith, of the island of St. Croix.

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of ingenious experiments on the wounds of intestines. The conclusions he has drawn we shall quote in his own words.

"It appears from the result of my experiments on dogs, that not only the intestine may be returned into the cavity of the abdomen, but that the ligatures may be cut off and returned with the intestine, as was observed by Mr. Thomson of Edinburgh, and that we need not be under any apprehension of their being discharged into the cavity; for, by some process of the animal economy of which we are ignorant, the ligatures have, in every instance, either been discharged with the fæces, or been found loosely attached to the *internal* coat of the intestine.

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the cure is complete, and shows the necessity of wearing a truss during the rest of his life.

The object of some writers to produce a radical cure by tying the mouth of the sac, Mr. C. considers as ineffectual and dangerous. Although the opening of the peritoneum is shut up by such an operation, the dilatation of the abdominal ring must ever remain open.

OF LARGE HERNIA.

Under this head our author records two important cases of large hernia, in one of which the bowels were reduced, after dividing the stricture, without opening the sac; in the other the sac was opened. The first patient recovered, the second died.

These cases, and the conclusions which our ingenious author has drawn from them, present to practical surgeons a point worthy of their mature consideration, Whether ought we, in general, to open the hernial sac? This mode of operation was first proposed by Dr. Monro, sen. and several cases are related in the appendix to his work on the bursæ mucosæ, where it was performed most successfully.

OF SMALL HERNIA.

Our author remarks, that it is by no means unfrequent to meet with cases of hernia where the hernial sac is so small as not to extend through the abdominal ring; and as in such there is little appearance of external tumor, the disease is very apt to be overlooked by the patient and surgeon, and some other cause assigned for the series of symptoms. The manner of operating in this form of the disease differs from that in the common scrotal hernia: the incision is to be made parallel to the direction of the spermatic cord, and the stricture will be found at the internal ring.

OF INGUINAL HERNIA ON THE INNER SIDE OF THE EPIGASTRIC ARTERY.

This variety of hernia, says Mr. C. has now very often fallen under observation, and has been for more than 25 years

described in the lectures delivered at St. Thomas's and Guy's Hospitals.

The following note, which is translated from Rougemont, the author of the French translation of Richter's treatise on hernia, will also show that this form of the disease has not only been *long* known and *well* understood on the continent, but the same mode of operation as Mr. Cooper mentions has been adopted.

"After all these considerations, I conclude with Mr. Dessault, that the epigastric artery in inguinal hernia is commonly placed near the *internal* angle of the ring, and rarely towards the *external*. The cases where that artery is placed at the external angle of the ring in inguinal hernia are very rare, and they do not happen unless when the viscera escape on the inner part of the ring, and then the cord is placed to one side, and a little behind the sac. I have had occasion, two years ago, to observe this distribution on a dead body; I preserved the preparation for several months, and showed it to several professional men."

Afterwards he adds,

"I believe, after what has been said, that we may be allowed to conclude that we run less risk of wounding the epigastric artery in cutting upwards and outwards, than in cutting upwards and inwards; that, in order to know exactly the situation of that artery, we ought to become acquainted with the relative situation of the spermatic cord and hernial sac; and, supposing that this is impossible, *we ought to make the incision directly upwards, through the middle of the superior edge of the ring.*"

OF CONGENITAL HERNIA.

The author, under this head, has nothing particularly interesting to communicate, except a case described by Mr. Foster, where, on dissecting carefully through the tunica vaginalis of the cord near the ring, a fluid escaped. "I then (says Mr. F.) continued the incision to the bottom of the scrotum through the tunica vaginalis of the cord, and the tunica vaginalis testis, which I found to be one cavity, the edges of which being turned back on each side exposed a hernial sac pendent from the ring, and descending towards the testicle." Mr. Cooper adds, "The idea I have formed of this case is,

that the tunica vaginalis, after the descent of the testis, became closed opposite to the abdominal ring, but remained open above and below it. That the intestines descended into the upper part, and elongated both the adhesion and tunica vaginalis, so as to form it into a bag, which descended into the tunica vaginalis below the adhesion, and becoming wide at its neck, though narrow at its fundus, it received a portion of intestine, which was too large either to be returned into the abdomen, or to re• in its functions, whilst it continued in the sac."

The work is embellished with eleven plates, illustrative of the anatomy and pathology of hernia. These and the letter-press are in very large folio, and although this size adds to the splendor of the book, it renders it so importable, and so teazing and difficult to read, that we cannot help thinking, that if the same materials had been printed in a different form, and sold at a more moderate price, it would have been more generally useful, and more within the reach of the great body of readers.

As drawings, we cannot help regretting that, situated as Mr. Cooper is in the focus of the arts, he had not been fortunate enough in employing a more able draughtsman. To us they appear to be executed with great stiffness, formality, and labour; they seem not as they were drawings from the subject, but as copied from heavy brass casts.

When we compare them with Camper's drawings on the same subject, our English artist is completely eclipsed. Camper, with all the spirit and power of a master, expresses, with a single well chosen line, what Kirtland tries to do by a hundred.

As engravings, they are well executed, but they are loaded and heavy with much unnecessary labour and superfluous work. They are far from being the *chef-d'œuvres* of Heath, and seem to be indebted to him for nothing but his name.

These remarks, however, apply only to the plates as works of art; with regard to their anatomical accuracy, we believe them to be true representations, and certainly they will ever be considered as a useful addition to surgical anatomy.

From this outline we have given of Mr. Cooper's work, we hope our readers will be enabled to form a general idea of its contents, and to appreciate its merits. The attentive practi-

tioner will, we venture to say, find in it much useful practical information; and the opinions and scattered observations of former authors, and of illustrious teachers, are so well arranged and so judiciously collected, as sufficiently to recommend the work to the careful perusal of every description of medical men.

The Anatomy and Surgical Treatment of Crural and Umbilical Hernia, &c. &c. By ASTLEY COOPER, F. R. S. *Honorary Member of the Medical and Physical Societies of Edinburgh, Lecturer on Anatomy and Surgery at Guy's Hospital. Illustrated by Plates.* Part II. 90 pages, atlas folio. London, 1807.

[From the Edinburgh M dical and Surgical Journal for April 1808.]

In our Journal for April 1806, we took an opportunity of giving a general view, and short analysis of the first part of Mr. A. Cooper's work on Hernia; and we now have the satisfaction of performing the same duty to the second part, which fulfils his promise, and completes his work on this interesting subject. In this volume Mr. Cooper comprehends the anatomy and treatment of the crural hernia, of the umbilical, ventral, pudendal, perineal, thyroideal, cystic, phrenic, mesenteric, and mesocolic herniæ, and of the strangulated intestine.

We formerly remarked the success of Mr. Cooper's investigations in the anatomy of the *inguinal ring*, and he seems to us to have been equally diligent, and no less successful in the dissections which he has now given of the *crural ring*. The complicated structure, however, of this part of the body, but, above all, the imperfection in the language of anatomy which is generally adopted, renders it extremely difficult to describe these parts with clearness and precision. Indeed, in no part of anatomy could the necessity of adopting the ingenious and precise language of Dr. Barclay be more strongly urged; for, notwithstanding the patient investigation Mr. Cooper has bestowed, his description is not altogether perspicuous, and it is with difficulty understood, unless by those who have the parts lying before them, or who are very familiar with dissection. From an aim at too great minuteness, we are convinced that the

structure of these parts has been made to appear much more complicated than it really is; and the insatiable desire which several anatomists of the present day have shown to multiply fasciæ, has at last been carried so far, and indulged to such a degree, that it appears as if no one believed himself entitled to any claim of excellence, unless he has added one to the list; just as many young surgeons conceive themselves incapable to perform an operation until they have contrived some new instrument: although it may in general be remarked, that the best practical surgeons invent fewest instruments, and the most experienced anatomists make the fewest discoveries.

In the anatomical part, Mr. Cooper first gives a description of the bones, then of the soft parts of the thigh which are adjacent to the cavity of the abdomen, and afterwards of those which shut up the abdomen from the thigh.

He describes one fascia, called the *superficial* fascia, which lies immediately underneath the skin and fat, extends over the tendon of the external oblique muscle, adheres firmly to the crural arch, passes downwards upon the absorbent glands of the groin, and extends down the thigh. Another fascia, called the *fascia lata*, lies underneath the former, and has two origins, one from the rounded edge of the crural arch, along the space between the tuberosity of the pubes and the anterior spinous process of the ileum; the other from the ligament of the pubes at the insertion of the external oblique muscle, extends over the pectineus and triceps muscles, and having united with the first mentioned part, under the saphena major vein, the united portions extend over the thigh, embracing the muscles, and forming an external covering to the femoral vessels as they emerge from the abdomen.*

The abdomen is described to be shut up from the thigh by two fasciæ, the *fascia transversalis* and the *fascia iliaca*. These, however, ought properly to be considered as only one expansion, which covering the transversalis, the psoi, and iliac muscles, and lying underneath the femoral vessels, passes down

* These two portions of fascia, when united, do not form one plane surface, but give a covering to the hollow space formed by the pectineus and the tendons of the psoi and iliac muscles, as may be seen accurately represented by Mr. Cooper in his former volume.

in the shape of a funnel through the crural arch, and forms the *crural sheath*. The space called the *crural ring* is also within this sheath, and is situated between the crural vein and the crescent-shaped edge of the insertion of the external oblique muscle into the pubes.

Besides passing down to form the crural sheath, the fascia which lines the muscles of the belly, or that portion called *fascia transversalis*, is also said, by Mr. Cooper, to extend over the crural ring. In the dissections we have made, we have never been able to demonstrate such a fascia. The crural ring, in most people, is filled up by an absorbent gland, or by a number of absorbent vessels and fat, so that, in these, no such fascia can exist. Where there are no absorbent vessels or glands, the crural ring is covered by a continuation of the cellular stratum which lies between the peritoneum and the fascia lining the abdomen. If the finger is pressed upon the crural ring, it may be passed from one half to three fourths of an inch towards the thigh within the sheath; and there is no aperture in this part of the sheath, except the minute cribriform holes for the passage of the absorbent vessels, or a single hole if they pass in a cluster. When the finger is thrust down through the crural ring, the lunated or semilunar edge of the fascia lata may be distinctly felt.

Crural Hernia.—After accurately describing the symptoms remarked by the patient, Mr. Cooper mentions that this hernia is generally found to be a small circumscribed tumor, about the size of the last joint of a finger, situated under the crural arch, about an inch on the outside of the tuberosity of the pubes, and lying in the hollow between this process and the crural artery and vein.

"As the tumor enlarges, instead of falling downwards like the inguinal hernia, it passes forwards, and often turns over the anterior edge of the crural arch, this being the direction in which there is the least resistance. As it proceeds, the swelling increases more laterally than upwards or downwards, so that it assumes an oblong shape, the longest diameter being in a transverse or horizontal direction.

"In the female it is generally very movable, and being soft, and the skin not being discoloured, it has the appearance merely of an inguinal tumor of one of the absorbent glands;

but, in the male, the skin is generally not so loose, the swelling not so distinctly circumscribed, and the tumor appears buried more in the substance of the thigh.

"The direction in which the crural hernia passes is obliquely inwards and forwards, and excepting at first, very little downwards, so that, in cutting into this tumor, the incision is made into its fundus. This is the general situation of the tumor, but it sometimes happens, that instead of crossing the thigh in the direction of the crural arch, it extends downwards along the edge of the crural vein and the vena saphena major.

"The crural hernia, when dissected, presents the following appearances: when the skin is removed, the superficial fascia of the external oblique muscle is laid bare, which, though it is of a delicate texture in its common state, when pressed upon by a hernia, becomes extremely thickened, and very distinct, more especially in a subject loaded with fat. Under this covering there is generally another fascia, precisely of the form of the hernia itself, and which it very closely embraces. A thin fascia naturally covers the opening through which the hernia passes, and descends on the posterior part of the pubis. When the hernia, therefore, enters the sheath, it pushes this fascia before it, so that the sac may be perfectly drawn from its inner side, and the fascia which covers it left distinct. The fascia which forms the crural sheath, and in which are placed the hole or holes for the absorbent vessels, is also protruded forwards, and is united with the other, so that the two become thus consolidated into one. If a large hernia is examined, this fascia is only found to proceed upwards, as far as the edge of the orifice on the inner side of the crural sheath by which the hernia descends, but, in a small hernia, it passes into the abdomen as far as the peritoneum, and forms a pouch, from which the hernial sac may be withdrawn, leaving this forming a complete bag over the hernia.

"In a small hernia, the fascia is thicker than the sac itself, but, by being gradually extended, it becomes thinner and less distinct; and, in one example of this kind from the female subject (which forms the subject of one of my plates), this and the superficial fascia have coalesced into one. I first observed this fascia in dissecting a male subject brought into St. Thomas's

Hospital in the year 1800, who had a strangulated crural hernia on the one side, and a reducible one on the other. I next saw it in the operation performed upon Mrs. Bispham, hereafter to be mentioned, and have ever since demonstrated it in preparations whilst delivering my lectures on crural hernia. It may be termed the *fascia propria* of the crural hernia. When this fascia is divided, a quantity of adipose membrane is found between it and the sac, and when this is cut through, the peritoneal sac itself is exposed. Behind the hernial sac is the fascia lata, and the sac rests in the hollow between that part of it which covers the crural vessels and that which passes over the pectineous and triceps muscles, so that the fascia lata is situated posteriorly to the hernia." P. 6.

From the description we gave of the crural ring in the sound state of the parts, we cannot agree with Mr. Cooper in saying, that a *thin fascia*, which naturally covers the crural ring is pushed forward before the peritoneum, and, by also pushing forward the sheath of the vessels, these two parts are united to form the *fascia propria* of the hernia. The *fascia propria* we have found in the cases which we have examined to be perfectly distinct, and the world is indebted to the persevering industry and the acuteness of Mr. Cooper for the knowledge of it. All we wish at present to remark is, that Mr. Cooper's explanation of the manner in which it is formed is not quite satisfactory; and we cannot help thinking, that instead of saying a fascia covering the crural ring assists in the formation of the *fascia propria*, we would say that it is composed of the cellular lamellæ and fat, which lie over the crural ring, and of the proper sheath of the femoral vessels. It is from a want of knowledge of this bag, (he very judiciously remarks,) in which the crural hernia is contained, that the great difficulty in the operation has arisen, and that surgeons have, in some instances, returned the hernial sac unopened, supposing it to be the intestine. We have seen repeatedly much confusion and embarrassment arise during the operation for femoral hernia, from the surgeon not being able to discriminate between the different coverings of the intestine and the intestine itself; and even in dissection after death, we have observed in old hernia the sac so much thickened, and so easily divisible into a number of

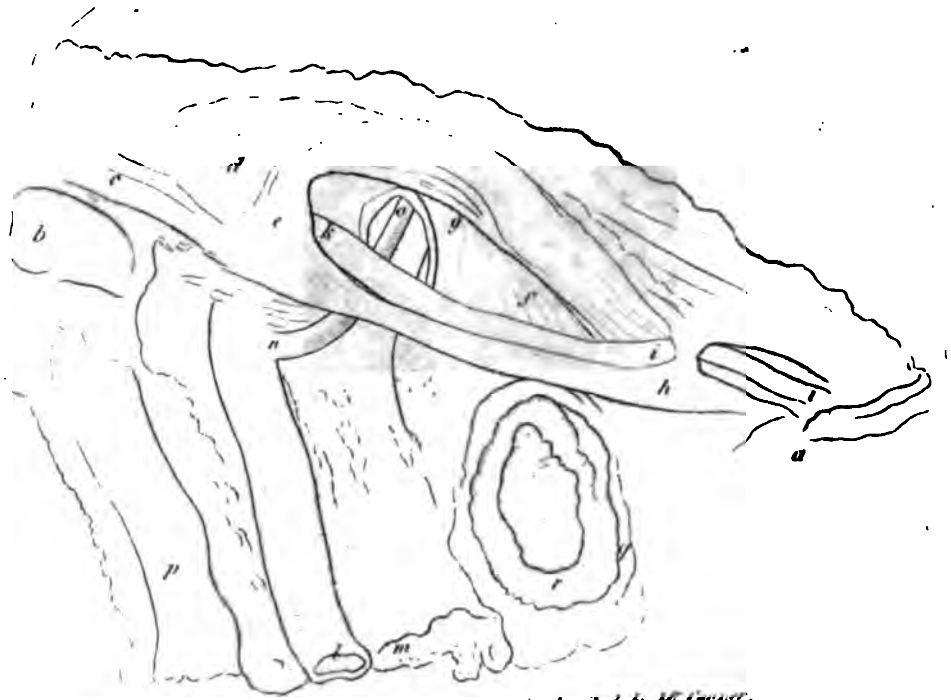
layers, and so vascular, that it was almost impossible to distinguish the peritoneal covering from a portion of intestine.

This part of the pathology of hernia appears to us of so much importance to the practical surgeon, that we have copied, in a simple outline, the sixth figure of Plate IV: It represents the dissection of a small crural hernia.

a Seat of the symphysis pubis.
b Seat of the spinous process of the ilium.
c Tendon of the external oblique muscle.
d Internal oblique and transversalis.
e Fascia of the transversalis. *f* Tendon of the transversalis.
g Inner portion of the fascia transversalis, passing to unite itself with the tendon.
h The crural arch. *i i* Round ligament.
k Round ligament passing into the abdomen.
l Crural artery. *m* Crural vein.
n Origin of the epigastric artery.
o Course of the epigastric artery behind the round ligament.
p Crural nerve. *q* Superficial fascia.
r *Fascia propria* of the crural hernia, the hernial sac having been drawn into the abdomen to show this fascia distinctly.

Our author next proceeds to give an account of the *diagnosis* of crural hernia from inguinal hernia, from psoas abscess, inflamed glands, varicose crural veins, steatomatous tumor, hydatid tumor; the cause of its greater frequency in the female; the contents of the sac, and the causes which most frequently bring on the disease. It is of much consequence not to confound the inguinal with the crural hernia, as both the taxis and operation are very different in each, and Mr. Cooper mentions several instances where this mistake occurred. It is by no means unusual to hear of a crural hernia having been mistaken for an inflamed gland, or an inflamed gland for a hernia; and this error is the more likely to happen, as in almost all cases of crural hernia, one or more glands are inflamed and swelled, and, in many instances, they so surround the herniary tumor as to prevent it from being felt.

Taxis.—It is of equal, if not more importance to attend to the position of the patient in making the attempt to reduce the fe-



Outline of a Hernial Sac shewing the Fascia Propria described by Mr Cooper.

moral than the inguinal rupture. "The shoulders should be elevated, and the thighs bent at right angles with the body; but even this posture produces little effect, unless, at the same time, the knees are brought together." In this position "the surgeon is to place himself over the body of the patient, and putting both his thumbs on the surface of the tumor, he is to press gently directly downwards, as if he was endeavouring to press the tumor into the thigh, rather than towards the abdomen. If this pressure is steadily kept up for some minutes, till the surface of the tumor is brought even with the line of the crural arch, the hernia may then be pressed towards the abdomen, and will return into that cavity." The truss for the crural hernia is much the same as that recommended for the inguinal; and the same general rules should be carefully attended to in its use and mode of application. It is only necessary that the pad be inclined more downwards, as, from the distance between the abdominal and crural rings, it is necessary that the bearing of the pad of the truss should be from one half to three fourths of an inch lower down than in inguinal hernia, and should also press on the upper part of the thigh. "For this purpose, it is best that the pad of the truss be bent somewhat downwards; and not as in the inguinal truss, contained nearly in a line with the spring."

Strangulated Crural Hernia.—In our review of the first part of Mr. Cooper's work, we took notice of the difference of opinion among surgeons, about the time the operation should be had recourse to. In general, the progress of the inflammatory symptoms is much more rapid in the crural than in the inguinal hernia, and it is therefore more necessary to guard against delay. Whenever a fair trial has been given to the usual means recommended for reduction, if the inflammatory symptoms are violent, the operation should be performed, the surgeon always keeping in mind, that the danger of the operation is in every case lessened in proportion as it is performed early; whilst, on the other hand, it is almost universally observed, that it is unsuccessful only when it is performed too late, or when insufficient means have been employed to remove the inflammatory symptoms, which had previously taken place.

"The incision of the integuments is to be begun an inch and

a half above the crural arch, in a line with the middle of the tumor, and extended downwards to the centre of the tumor below the arch. A second incision, nearly at right angles with the other, is next made, beginning from the middle of the inner side of the tumor, and extending it across to the outer side, so that the form of this double incision will be that of the letter T reversed.

"The first incision exposes the superficial fascia, which is given off by the external oblique muscle, and which covers the anterior part of the hernial sac; but if the patient is thin, and the hernia has not been long formed, this fascia escapes observation, as it is then slight and delicate, and adheres closely to the inner side of the skin. When this fascia is divided, the tumor is so far exposed, that the circumscribed form of the hernia may be distinctly seen, and a person not well acquainted with the anatomy of the parts, would readily suppose that the sac itself was now laid bare. This however, is not the case, for it is still enveloped by a membrane, which is the fascia that the hernial sac pushes before it, as it passes through the inner side of the crural sheath. This membrane, the fascia propria, is to be next divided longitudinally from the neck to the fundus of the sac; and if the subject is fat, an adipose membrane lies between it and the sac, from which it may be distinguished, by seeing the cellular membrane passing from its inner side to the surface of the sac.

"This is, in my opinion, the most difficult part of the operation, for the fascia propria is very liable to be mistaken for the sac itself; so that, when it is divided, it is supposed that the sac is exposed, and the intestine is laid bare; following upon this idea, the stricture is divided in the outer part of the sac, and the intestine, still strangulated, is pushed with the unopened sac into the cavity of the abdomen.

"The hernial sac being exposed, is to be next opened; and to divide it with safety, it is best to pinch up a small part of it between the finger and the thumb; to move the thumb upon the finger, by which the intestine is distinctly felt, and may be separated from the inner side of the sac; and then to cut into the sac, by placing the blade of the knife horizontally. Into this opening a director should be passed, and the sac opened from its fundus to the crural sheath."

It often happens, that this operation is sufficient to allow the strangulated intestine to be reduced, for it is now completely exposed, and it is much more under the command of the surgeon's fingers, than when enveloped in its different coverings. If, however, the contents of the sac are so much swelled, that they cannot be made to pass through the neck of the sac into the cavity of the abdomen, it becomes necessary to divide the stricture, and this, Mr. Cooper observes, may occur in three different situations:

1st, In the crural sheath, and semilunar or lunated edge of the fascia lata.

2d, At the posterior edge of the crural sheath.

3d, In the mouth of the hernial sac and fascia which covers it.

The seat of the stricture being ascertained by the introduction of the point of the finger, it is to be divided in a direction "obliquely inwards and upwards, at right angles to the crural arch," of a sufficient extent to liberate the intestine. But we may remark, that in every operation for hernia, of whatever kind, the business of the surgeon is, to divide the part forming the stricture and impeding the return of the strangulated intestine,—this must be divided; and if he divide no more, it is of little importance in what direction the incision be made. Indeed, we have seen cases where the stricture was so great, that it was with much difficulty that the nail of the surgeon's finger could reach underneath it, and he was obliged to divide it in any part where he could get the knife introduced and in any direction.

The varieties of the crural hernia, which Mr. Cooper mentions, arise either from the superficial fascia giving way, so as to allow a portion of the tumor to pass before it, thus dividing the tumor into two parts, or from the tumor not quitting the sheath for the crural vessels. Another variety occurs, where the tumor is formed "in part within the sheath, and also in the common way;" in one case, the author found the umbilical artery on the outer side of the sac. Another, and perhaps the most important variety, is, where the obturator artery comes off in a common trunk with the epigastric, and passes round the neck of the hernial sac.

This distribution of the arteries is by no means unusual; but,

in general, the common trunk of the two arteries is so short, that the obturator passes behind the sac. Mr. C. mentions only three cases, one which occurred to Dr. Barclay, and two which were communicated to him by Mr. Wardrop, where the common trunk was so long as to surround and pass anterior to the mouth of the sac.

Mr. Cooper, after making some very pertinent remarks on the operation inwards, or that proposed by Gimbernat, and his reasons for not adopting it, concludes the subject of crural hernia by illustrating the whole with very instructive cases.

Mr. C. has added a communication from Mr. Burns, demonstrator in anatomy at Glasgow, in which, besides remarks on some peculiarities in the structure of the parts concerned in hernia, he gives an account of a case of abdominal inguinal hernia, similar to those mentioned by Petit and the late Dr. Hamilton of Glasgow. In all these cases, the intestine entered the *internal* inguinal ring, and, instead of passing along the inguinal canal, and coming out at the *external* ring, passed directly through the abdominal parietes, opposite to the internal opening. Such cases occur either in consequence of an original deficiency in the number of transverse tendinous bands, which unite the columns of the tendon of the external oblique, or from their being lacerated.

Umbilical Hernia.—After having taken notice, that the peritoneum is as complete behind the umbilicus as at any other part of the abdomen, our author concludes, that the sac, in umbilical hernia, is formed of peritoneum, except in cases where that membrane is lacerated. He then describes the appearance of the disease, and the varieties to which it is subject; its frequency in children, its diagnosis, and the various causes from which it most usually proceeds. These are pregnancy, obesity, ascites, large funis, or a deficiency in the abdominal muscles.

In attempting to reduce this species of hernia, the same attention is necessary in relaxing the abdominal muscles, by elevating the shoulders, raising the pelvis, and bringing the thighs at right angles with the trunk.

"In making the reduction, the surgeon grasps the tumor in his hand, and directs it upwards, as the opening which leads from the swelling into the abdomen is not directly in the

centre of the tumor, but a little above it, and he then kneads the neck of the tumor with the finger and thumb of the other hand; but if the swelling is small, and projects only from the abdominal muscles to the navel, without spreading on each side, a direct pressure may be made upon the surface of the tumor with the thumbs, so as to force it into the cavity of the abdomen."

After the hernia is reduced, a proper bandage should be applied; and Mr. C. recommends one exactly similar to that used for the inguinal rupture. This, we know from experience, answers most completely, and it is by far the most simple contrivance. The spring should reach from the pad round to the back, a little further than the spine.

When the hernia is very large, especially when the belly is also pendulous, broad thin pads are recommended, which both keep back the hernia, and give a support to the abdomen. In infants, Mr. C. has found the following plan answer:—

" A small section of an ivory ball is laid on the navel; over this is to be applied a piece of adhesive plaster, of the size of the palm of the hand, and a linen belt over the whole round of the abdomen. As soon as the child begins to walk, two thigh straps are to be added to the bottom of the belt, to prevent it from slipping. If this application be carefully persisted in for a length of time, the cure of this disease is very sure to be effected."

Indiscretion, both in the quantity and quality of food, is the most frequent cause of strangulation in this species of hernia; and Mr. C. has often observed quantities of ham, and lumps of fat, pass through indigested in those cases where an artificial anus was formed.

Our author has found the tobacco glyster, as directed in Part I. of his work, to be a most useful remedy in the strangulated umbilical hernia. And when that fails, he advises the use of cold applications. The symptoms which render an operation necessary in this species of hernia are the same as those which occur in the other species. Mr. C. does not employ the common operation, by a longitudinal incision of the tumor, but has adopted the following:

" As the opening into the abdomen is placed towards the

upper part of the tumor, I began the incision a little below it, that is, at the middle of the swelling, and extended it to its lowest part; I then made a second incision at the upper part of the first, and at right angles with it, so that the double incision was in the form of the letter T, the top of which crossed the middle of the tumor. The integuments being thus divided, the angles of the incision were turned down which exposed a considerable portion of the hernial sac. This being then carefully opened, the finger was passed below the intestines, to the orifice of the sac at the umbilicus, and the probe-pointed bistoury being introduced upon it, I directed it into the opening at the navel, and divided the *linea alba* downwards, to the requisite degree, instead of upwards, as in the former operation.

"When the omentum and intestine are returned, the portion of integument and sac which is left at the upper part undivided, falls over the opening at the umbilicus, covers it, and unites to its edge, and thus lessens the risk of peritoneal inflammation, by more readily closing the wound."

In cases where the hernia is very large, and consequently where the risk of peritoneal inflammation is considerable, Mr. C. has, in several cases, instead of laying open the sac and the abdominal cavity, and exposing a large quantity of intestines, made a small incision of the integuments, at the neck of the sac, and divided the stricture.

In *Ventral Hernia*, whether the disease has been occasioned by an enlargement of any of the apertures of the bloodvessels, from original malconformation, from the ulceration of muscles, or from wounds, the same general rules are laid down as those in the other species.

The subject of *Vaginal*, *Pudendal*, *Perineal*, *Thyroideal*, *Cystic*, *Ischiatic*, and *Phrenic Hernia* our author discusses as minutely as the subject will allow; but these species occur so seldom, and as Mr. C.'s information would not admit of a short analysis, we must refer those to the work itself who wish for full information on these subjects.

Our author mentions one remarkable case of *Mesocolic Hernia*, in which the whole of the small intestines were contained within a sac formed between the layers of a dilated portion of the mesocolon.

He has also found the intestines strangulated, from apertures in the omentum, mesentery, or mesocolon,—from adhesions having formed in consequence of inflammation, leaving an aperture in which a portion of intestine becomes confined, by membranous bands, formed in the mouths of hernial sacs, which becoming elongated by the frequent protrusion and return of the viscera, surround the intestine, so as to strangulate it within the abdomen when it was returned from the hernial sac.

Mr. C., in completing this work, has added many useful and ingenious observations to the anatomist, and he has contributed so much to a very important part of surgery, that he has left little more to be done in it. A most commendable zeal in a profession which interests the happiness of so many individuals, seems to have actuated this gentlemen in pursuits where much difficulty and many obstacles opposed his progress, and which nothing but indefatigable industry and perseverance could overcome.

The anatomical description, and the most remarkable morbid appearances are very fully illustrated in seventeen engravings. The opinion we formerly gave of these as works of art, we might again repeat, as it may apply to the greater number of those contained in the volume before us. Although, however, the drawings do not breathe the spirit of a great master, yet they are executed with great neatness and truth, and much care seems to have been taken in the choice of proper subjects. We ought, however, to caution our readers against one error into which Mr. C. has inadvertently fallen. He wishes the plates to be referred to for taking *measurements* of the relative situation and distances of parts. Had they been accurate *maps* instead of *views*, this might have been done, but in the art of drawing it is impossible; for as the different parts are seen in perspective, their apparent distance and situation must vary according to the situation of the eye of the observer.

In the notice we have taken of this valuable work, our chief object has been to extract some of the important information and the judicious practical lessons it contains, not to offer many critical remarks; and the analysis we have made, will, we hope, rather than satisfy the arduous in their professional inquiries, induce them to consult the work itself.

Anatomie et Physiologie du Système Nerveux en général, et du Cerveau en particulier, avec des Observations sur la possibilité de reconnoitre plusieurs dispositions intellectuelles et morales de l'homme, et des animaux, par la configuration de leurs têtes. Par F. J. Gall *et* G. Spurzheim. *Premier Volume. Anatomie et Physiologie du Système Nerveux en général, et Anatomie du Cerveau en particulier.* 4to. pp. 351. Paris, 1810. Atlas en folio, contenant dix sept planches.

[From the London Medical Review for July, 1811.]

The anatomy and physiology of the brain have of late engaged much of the attention of the anatomists and men of science on the continent. Alleged discoveries of facts not before noticed in the structure of this organ, together with a new mode of explaining its functions, have already formed the basis of a complete system of physiology of the brain, in which the special functions of the several parts of which it is composed are regularly determined and explained; and which includes, as essentially connected with these functions, the natural history of our faculties, our dispositions, in short of all our intellectual and moral qualities.

Of the philosophers who have hitherto reasoned on the powers of the brain, the greater part have contented themselves with considering it as the common centre of sensation, and the general organ of the will: whilst others, without possessing any accurate knowledge of its structure, have ventured to assign particular functions to particular parts, and have fallen into the greatest absurdities. Of the numerous anatomists, and physiologists who have diligently examined this organ in various states both of health and disease, the majority, avoiding metaphysical discussions, have confined themselves to attempts at illustrating its importance in the animal system: some, and those of the greatest eminence, struck by the observance of various phenomena in the course of their researches, were convinced of the existence of different qualities both physical and intellectual as connected with special parts of the brain, but their notions on the subject were so confused and their judgments so much warped by the metaphysical opinions of the day,

that they had the effect rather of throwing the subject into shade, than of diffusing upon it any light which might direct the attention of future inquirers. There are yet a few who think that every thing has already been done; that after the numerous dissections which have been made, and descriptions which have been given of the structure and appearances of the brain, nothing remains to be discovered, and that all further attempts at investigating its functions must be idle and full of metaphysical uncertainty.

It is natural to suppose that a doctrine which struck at the root of the commonly received opinions, and which professed and promised so much, would be received with jealousy, and freely canvassed by those, whose self love it so sensibly touched, and against whose theories it so strongly militated. We find accordingly that the opinions of Gall, on their first promulgation in Germany, whilst they had various supporters, who adopted them without examination and bestowed on them unbounded praise, met also with strenuous opponents, who supporting themselves by some reasonable objections to subordinate parts of the system, scrupled not to reject the whole as false and absurd. Others unable to oppose facts by argument, broke out into lamentations on the horrible moral consequences which must follow their adoption. The greater number of the adversaries of this physiology, without at all troubling themselves to examine the subject, attacked it by ridicule, in matters of science the weakest of all the weapons of criticism.

The doctrine of Gall which excited so lively an interest on the continent, has been received so coldly in this country, that but few we believe have any accurate knowledge of the principles on which it is founded: sure we are that no one has made any attempt to investigate it. And whilst his peculiar physiological opinions have been but very imperfectly understood, and mentioned only to be laughed at, his successful labours as an anatomist, the basis of his physiological opinions, have been wholly overlooked. In France his pretensions have met with a more favourable reception; and his anatomical observations, more particularly, have been patiently and deliberately examined by men who hold the first rank amongst the anatomists and physiologists of the present day.

The doctrines of Gall with regard to the structure of the nervous system, and more particularly of the brain, totally change the common ideas and methods of investigating the subject. We should observe here however, that he does not pretend to have discovered many new facts; the chief merit that he claims consists in the connexion which he has been the first to establish between known facts, and in the general conclusions which he has deduced from them. His doctrine concerning the special functions of different parts of the brain is in many respects new, ingenious, and important. Its intent is to prove that there are in man and animals, innate qualities and dispositions, connected with certain organs which are situated in the brain: that the brain is not so much the general organ of animal life, as the place in which the individual organs are collected, each innate quality having its special organ: that these organs show themselves on the surface of the brain, and form eminences, marked by corresponding risings of the cranium; and that from an observance of these last, we may under certain limitations, determine the existence of such organs, in a greater or less degree of development, answering to the greater or less degree of energy in the intellectual or moral disposition with which they are connected. Its merits must rest on the number and importance of the facts which can be brought forward for its support. We shall not at present, however, enter on this part of the subject, as the volume which is to contain his own detailed account of the opinions is not yet before the public. The judgment already passed on them has been made up from accounts published by some of his pupils, none of them entirely acknowleged by himself.

It is now more than twenty years since Dr. Gall first made known his peculiar opinions, which by his own account, he began to form very early in life, from observations made on the dispositions and talents of his school-fellows. As he advanced he lost no opportunity of repeating, and of trying the truth of his remarks, and became more and more convinced of their correctness. Aware that any doctrine explanatory of the functions of the brain must necessarily be very imperfect, if not strictly and intimately connected with the structure of the organ, he entered on minute anatomical ex-

aminations of its substance. His opinions, as he gradually made them known, became the subject of much discussion in Germany, where they have been the cause of a warm literary warfare.

It was not long before Dr. Gall, perceiving the interest his doctrine had excited, entered on a regular course of lectures on the physiology of the brain, in which he enlarged more particularly on the existence of special organs, for different intellectual and moral qualities, and on the possibility of ascertaining their existence, by the form of the head, in the living individual. His lectures given at Vienna, were exceedingly popular, and very numerously attended; when he was interrupted by an order from the government, and prohibited from lecturing before any but foreigners. The reason assigned was, that his doctrine was dangerous in its tendency, and led to materialism and atheism. Weak indeed must that establishment have been in church and state, which could be seriously alarmed by the harmless doctrines of Dr. Gall. They savoured no more of materialism than any other of the numerous physiological systems of the functions of the brain, which had never probably been discussed out of the precincts of the schools, certainly had never been thought dangerous enough to require the interference of the rulers of an empire; and this too, in the nineteenth century.

Gall soon after quitted Vienna, in company with Dr. Spurzheim. They visited the principal cities in the North of Germany, giving public lectures, conversing with men of science, inviting criticism, and always diligently labouring to add to their store of facts. They every where met with a most flattering welcome; and their designs were forwarded in every way by sovereigns and men in power, as well as by the learned, and artists of every description. Among many distinguished physiologists who had now taken up the subject as one of considerable interest, we find Hufeland, Walther, Loder, Reil, and Sœmerring; the two former as opponents, the latter as supporters of most of his opinions. All of them men of extensive knowledge, and high authority in anatomical and physical science. It is by such men that the question must be finally determined.

We next find Gall and his zealous assistant Spurzheim at Paris, in which place also they have continued their labour of collecting and arranging materials for the present work. They have given repeated demonstrations of their anatomical discoveries to all who expressed any desire, or showed any intention of investigating the subject, and have left no one to complain of a want on their part either of liberality, industry, or talent. In the beginning of 1808, they presented to the Institute, in matters of science one of the first tribunals in Europe, a memoir intitled "Recherches sur le Systeme Nerveux en general, et sur celui du Cerveau en particulier." It was referred for examination to MM. Tenon, Portal, Sabatier, Pinel, and Cuvier, who drew up in consequence a most elaborate and excellent report, which contains within a short compass the essence of Gall's opinions relative to the anatomy of the brain, together with many valuable remarks on them by the reporters. This report has been translated, and published in the fifth volume of the Edinburgh Medical Journal, and we cannot too strongly recommend it to the notice of all such as are desirous of inquiring into the structure and probable functions of so important an organ.

The reporters in speaking of some of the motives which induced them to undertake an examination of the memoir, say, "the consideration of the importance of the functions of the nervous system, and of the ignorance which still exists, concerning several points of its structure, notwithstanding the numerous examinations to which it has been subjected, fixed our determination. Whoever flatters himself that he is able to throw any light on a subject so interesting, and so obscure, is, in fact, entitled to be heard with attention, by a body such as ours; and we should be wanting in our first duty, if our examinations were not conducted with the greatest attention, and strictest impartiality." "We invited therefore Dr. Gall and Dr. Spurzheim to our conferences. They readily dissected the brain in our presence, and we dissected it before them; we afterwards reexamined by ourselves the observations they communicated to us; lastly, we endeavoured for a time to adopt their manner of veiwing the subject, and to give a clear and precise abstract of it, which we submitted to them, that

we might be satisfied whether we had rightly comprehended their ideas."

Indeed, they appear to have taken every means of ascertaining what was new, or just, in the ideas of the authors of the memoir, and to have used every precaution before they formed their opinion. This opinion is, with but few exceptions, much in favour of our authors, allowing them the merit of having recalled to our attention, and again demonstrated neglected facts, of having discovered new ones, and of having drawn some just and important conclusions from both.

The memoir above alluded to has since been published by its authors with some remarks on this report. It is confined wholly to anatomical investigations.

We confess that it has excited our surprise, to find so little notice taken of the memoir, or of the report, by the men of science, or by the anatomical teachers of this country, the latter of whom, still go on slicing the brain as they were wont to do. It has always appeared to us, that the method of demonstrating the brain at present commonly employed in our anatomical schools, is defective in every sense. What idea could possibly be formed of the appearance or structure of the thoracic or abdominal viscera if the dissection were performed in a similar manner? Who could divine the functions of the heart or stomach from a demonstration of thin horizontal slices? The principal effect of this practice is, to puzzle the student with hard names of parts imperfectly shown, the structure and relations of which are neither pointed out nor sufficiently explained. Add to this, from the manner in which the subject is generally treated, the student is soon impressed with the idea that a minute knowledge of the anatomy of the brain can never be of much service to him in the practice of his profession, and that any time spent in acquiring it is but so much thrown away.

Of the work now before us the first volume only has been received in this country: we believe it is the only one yet published. It contains a very detailed description of the nervous system, and of the brain; and is accompanied by a folio volume of plates, executed in a most masterly and finished manner. The second volume will contain the general principles of the

physiology of the brain; and the third the doctrine of the different organs.

It has been objected to Gall that he cautiously abstained from printing any of his opinions, and that he depended for popularity on the flattering accounts of prejudiced, and enthusiastic pupils. To this he answers in the preface to the present work: "Every one expressed a desire of seeing me put to the press my physiology of the brain; I was much inclined to do it, but from day to day a wider field opened to my view, and questions and problems continually multiplied. It is for these reasons, that, notwithstanding repeated solicitations, and many keen provocations, I always endeavoured to put off the time of publication. If I am enabled to recompense the reader by a more abundant harvest of observations, and by more positive conclusions, it is owing to this intentional delay, and to my regard for science."

A considerable part of the present volume is occupied by a long desultory preface, and an introduction. The former notices the various labours of the authors, and the obstacles which they had to overcome; and touches slightly on some of their physiological opinions. It betrays here and there an inclination in the composers to give us some fine writing, but at the same time contains many philosophical views of the functions of the nervous system.

"In this volume our principal object is the anatomy of the brain. But in order to give a connected view of the whole, and to enable the reader to understand more readily the laws of this the most noble part of the nervous system, we shall begin by treating of the more simple and lower order of systems, the ganglions and plexuses of the abdomen and thorax, and the nervous systems of the vertebral canal and of the senses. As we do not confine ourselves to give merely a detail of the mechanical structure of the brain, but connect with it our views of its physiology, we shall also join to our anatomical description of the inferior nervous systems some account of their functions."

"It has been said indeed that our doctrine on the structure of the brain has no necessary and immediate connexion with our doctrines of its functions, and the influence of the pro-

portionate size of its different parts; that all we have advanced on the structure of this organ might be equally either true or false, without thereby affecting the conclusions that may be drawn as to our physiological opinions. But this is separating anatomy from physiology; it is destroying all the relations between organs and their functions. It is certain that any doctrine of the functions of the brain would be false, if in contradiction to its structure: at the same time we do not pretend to explain any faculty by anatomical structure. Anatomy may serve to confirm, and to explain physiological truths; but it is seldom that it leads to a knowledge of functions, especially where the brain is the subject, the parts which have so much apparent resemblance to each other. Whoever is convinced that the structure of the parts of the brain has a necessary and immediate relation to their functions, will perceive that it is natural to unite these two objects, and to consider them as forming parts of the same body of doctrine. Since we attach so much importance to an exact knowledge of other organs, why not still more to that of the nervous system? Can there be in all the domains of natural history a study which concerns us more nearly than that of the organization of the brain?

"In anatomy our principal aim is to make known the organization of the nervous system, and particularly that of the brain; directing our attention in the first place towards the discovery of the organic laws of the nervous system; and in demonstrating them to set off from the more simple systems, and to ascend by degrees to the more complicated. In physiology, we endeavour, indeed, to determine the primitive faculties of the mind, the situation, and the external marks of their special organs; but however important a knowledge of these may be to the philosopher, or the physician, we are far from confining ourselves to this single object, only regarding it on the contrary as one of the best means of ascertaining with exactness the functions of the brain and of its several parts. Consequently we direct our attention in an especial manner to researches on the faculties of irritability, of sensibility, of voluntary motion, as well as on the functions of the senses, the will, or thought, as far as any of these qualities is dependent on conditions of matter. On every occasion we take

into our account, education, morals, legislation, the art of healing in general, as well as the knowledge and treatment of mental and moral affections."

We have allowed the authors to speak for themselves, that our readers might see the extensive route they have marked out, and the importance of the subjects which they intend to examine. We must excuse a little vanity in the following account of the opposition made to their doctrines, and of their final triumph. It is but a small part of what they say on this subject; we have many bitter complaints of the prejudices and jealousy of rival anatomists, as well as self-congratulations on the impregnability of the principal parts of their doctrines.

"We have hitherto taken notice of all the objections made to our assertions, which have come to our knowledge, and we shall always do the same. The only thing we regret is that our antagonists do not take time to understand our doctrine before undertaking to refute it. The chief part of them attack rather some absurdities which have been attributed to us than our real opinions; a manner of proceeding which betrays motives of no honourable source rather than a zeal for truth. Any discovery however, would remain very incomplete, if it obtained universal assent too readily. For the success of its author, nothing is required but that he live under a government which does not believe that the safety of the universe may be endangered by publishing natural truths. It is the coward only who is intimidated by difficulties and trifling persecutions. The man who is conscious of being the supporter of a new truth, feels a new spur in every difficulty, in every obstacle a new subject for triumph. It is only by such opposition that he is enabled to view the subject in all its bearings; it is only by resolving doubts, and answering objections, that he arrives at the point of placing the principles of his doctrines in security against the reach of any attack. It is thus that we have succeeded in placing in the rank of confirmed and demonstrable knowledge, the discoveries which we have made on the structure, the arrangement, the connexions, and the relations of different parts of the brain; on the use of its two substances, as well as on the origin and direction of the nervous fibres. We believe that we have introduced order,

unity, and life into a study in which hitherto there has been so much disorder. Where nothing was seen but mechanical forms, we shall show the material apparatus of the functions of the mind. Without presuming to understand the essential principle of our faculties, we shall demonstrate the corporeal conditions on which these faculties depend."

We were struck by some true, and eloquent observations on the peculiar advantages and opportunities, enjoyed exclusively by medical men, of studying human nature; and on the probable good which might be hoped for from their experience, in regulating civil institutions. It is by such views that we are led to value more highly, and to allow more readily the importance and utility of an honorable profession.

In the introduction we have an historical account of all the various opinions which have been held on the physiology and anatomy of the nervous system; some observations on the importance of studying its functions, and on the obstacles which retarded this branch of knowledge. In the concluding paragraph we find a review of their proceedings, which we insert, in order to give our readers a favourable impression of the merits of men who have laboured so indefatigably. Without particularizing their anatomical investigations, they say—

"In order to remove every obstacle, and employ every means within our reach, we have combined with our previous researches, and with the fruits of many years' labour, all the information we have been able to collect in our travels. Our intercourse with a great number of distinguished men, and an observance of their predominant qualities, have furnished us with many luminous ideas. We have profited by the observations made by our auditors, and by our friends, and by the objections made by sceptics, and by our adversaries. It is thus that we are arrived at the knowledge of many difficulties and many defects in our doctrine. We have at the same time studied without ceasing the organization of men of great talents, and that of men of low talents, in order, by the comparison, the better to hit on the difference between them. We have collected innumerable facts in our visits to schools and the great establishments for education, in the hospitals for foundlings and for idiots, in the houses of correction and in prisons, in courts

of justice and at the scaffold. We have added very largely to our collection of busts, and of the skulls of men and animals. We have examined a great many anatomical cabinets, &c. And lastly we have compared our opinions and our maxims with those of ancient and modern authors."

"After the employment of all these precautions we hope to have the credit of being thought sufficiently prepared to treat of the anatomy and physiology of the nervous system, as well as the history of these sciences. We shall attach ourselves more particularly to the brain, as being of the most delicate and most perfect organization, and as the immediate instrument of the soul. These labours appear to us of the greater importance, as it is in this part that the knowledge of anatomists, physiologists, and philosophers, is the least advanced. For the same reason we think we have some claim to the indulgence of our cotemporaries, and of posterity, for the errors and imperfections that may be discovered in our works. They will not be attributed to a want of application, to the omission of any necessary means, but solely to the incompetency of our talents, and to the shortness of life. We shall ourselves point out some deficiencies in our doctrine; without doubt our successors will discover others, which at present we can see but in the distance."

Hitherto we have done nothing but introduce our authors, and we have been thus copious in our observations, to enable our readers to form a just estimate of their pretensions and labours. We have been anxious to vindicate the name of Dr. Gall from that ridicule with which, in this country, it is generally associated; to represent him not as the idle dreamer of craniology, but the author of deep and erudite researches into the anatomy and physiology of the brain. In our next, we shall give some account of the store of anatomical facts contained in the present volume.

[*Same subject continued.*]

[From the London Medical Review for October, 1811.]

It is so rarely our lot to meet with a book containing accounts of discoveries in the structure and functions of any part of the

human body, that we believe many of our readers would agree with us in entering at length into an examination of the important contents of the present volume. But it would be fruitless to endeavour to discuss the many subjects of which it treats within the necessarily short limits of a Review. It contains such detailed descriptions of different anatomical appearances; so many physiological disquisitions; so copious a history of the opinions of every eminent anatomist, as well as those more peculiarly their own; and such elaborate answers to the objections made to many of their conclusions, that it will be impossible even to give a view which shall include every point. We must confine ourselves to noticing the most prominent, and most interesting features of the discoveries and opinions contained in the present work.

Our authors begin with the examination of the intercostal or great sympathetic nerve, because in the more perfect classes of animals the ganglions and plexuses of the abdomen and thorax may be considered as forming the system of an inferior order of functions. Their opinions with regard to the origins of this nerve will wear no semblance of novelty to such as are acquainted with the writings of later physiologists. The opinion which regarded the brain as the origin and point of departure of all the nerves of the body, either immediately or by means of the spinal marrow, has been gradually departed from, in proportion as a better acquaintance with comparative anatomy, and a more accurate mode of reasoning in physiology have shown its inconsistency. Many anatomists have remarked in particular the impropriety of deriving the great sympathetic nerve from the brain, and have considered the ganglions as so many separate origins of it. Bichat has entered more at large on the same subject, and in establishing the ganglions as so many nervous centres, possessing each an independent action, or influence, exerted on the nervous filaments passing from them to different parts, has given us occasion to admire the clearness of his conceptions, and the expressive language by which his theory is at once supported and adorned. To strengthen these opinions, our authors remark that the nervous systems of the viscera of the lower classes of animals, who possess neither brain or spinal marrow, may be looked on as analogous to simi-

lar systems in the higher classes, in whom similar functions are carried on. They correspond to the nervous plexuses of the abdomen and thorax, and to the series, more or less interrupted, of the ganglions of the sympathetic nerve; admitting this, it is demonstrated that this apparatus exists independently and of itself; and that the nervous filaments which connect it with the nerves of the head and spinal canal are only branches of communication. We are certainly fully disposed to consider the nervous systems of the abdomen and thorax as so many distinct systems, having a reciprocal action on each other, and on the brain, and exercising different functions according as they give origin to different nerves.

The authors throughout this work make the greatest use of comparative anatomy. It is from analogies drawn from that source that many of their opinions originated. The manner in which it is brought forward in the present instance is ingenious, and the argument is well followed up. With regard to the knowledge derived from comparative anatomy in elucidating the structure and functions of the nervous system in general, we shall here for once observe, that although we must be cautious in admitting as proofs of an uncertain physiological opinion, examples taken from animals differing so widely as an acephalous molluscus and man, yet where the series of facts with respect to the apparent organization of parts can be clearly traced from the simple to the more compound, it must be allowed to constitute no inconsiderable proof. Nor can we see by what reasoning some physiologists are guided, who in investigating the structure of other parts of the animal system, constantly refer to comparative anatomy as a fruitful field of convincing facts, and as the surest means of avoiding errors in physiology: yet who, when the brain is the subject, will not for a moment allow any conclusions to be valid, though drawn from analogies as plain as those on which they build so much. Scarpa is allowed to bring forward the organ of hearing from the cuttle-fish, through all the different classes of animals, to explain its functions in man. We can see no reason why the same excellent plan should not be followed in the present case. There is nothing so very distinct in the brain and nervous system of man as to exclude all analogies drawn from other

living creatures. Our advancement in anatomical knowledge is at once stopped if deprived of such a clue. The more simple the structure of an animal, the more easy is it to examine its nervous system; the greater the probability of making, or of confirming discoveries, than if we examined only animals of a more compound order, in which so many more parts are intermingled with one another. In man, the brain, considered exclusively, is certainly more developed, more perfect in structure, than in any other creature; but we are led to the discovery of this perfection in structure by comparative examinations, in the same manner as we are led to discover the perfection of its powers. In other animals various parts of the nervous system are traced in stronger lines than in man, the whole apparatus more plainly developed, the nerves proportionally larger throughout, and their origins and terminations more distinct.

The authors proceed to investigate the functions of the systems of the great sympathetic nerve, and in conclusion give the following statement of their opinions on the nature and use of the ganglions. This we have extracted as expressive of their ideas on the origin of the nervous or medullary fibres; a point of their doctrine which has been warmly assailed, and which applied to the higher parts of the nervous system is the very keystone on which the main part of their opinions rests.

"We have observed in the inferior classes of animals two substances forming the nervous system; the greyish gelatinous substance, and the nervous threads. The viscera are provided with collections of this gelatinous matter, and we may distinctly see nervous threads arising from them, and passing to different parts where they are lost. And as these collections of the gelatinous substance form knots or swellings, they are called ganglions. The more considerable the mass of gelatinous substance, the stronger and the more numerous are the nerves which proceed from it. We suppose that the nervous filaments are originally produced in this substance, and that they are nourished by it. The same conditions being found in the plexuses and ganglions of the higher classes of animals we should look on these also as the origins of nerves. They are penetrated by numerous vessels, and exhibit every where the gelatinous substance, pulpy, unctuous, and of a reddish grey

colour. They take different forms; and are of different consistence according to the distribution of the cellular tissue. The gelatinous substance is sometimes generally diffused, and the nerves which arise from it form a plexus; at others, it is collected in smaller masses in the plexuses. The difference in form, consistence, and colour, shows that these qualities are not essential conditions in the nature of a ganglion; they simply indicate modifications of structure."

The second section includes the history of the nervous system of the vertebral column.

The term spinal marrow has been used in a vague manner by different authors. The natural limit, in giving a description of it, appears to be the edge of the foramen magnum. Accordingly in the present inquiry it is supposed to end at the lower extremity of the pyramidal bodies, although it is by no means inferred that the structure of the spinal marrow is changed immediately as it becomes contained within the cranium. It is to be wished that the term marrow were discarded, as it is not applicable either to the grey gelatinous substance which occupies the central part, or to the nervous fibres which form the outer part of the cord.

In common language the spinal marrow is spoken of as a prolongation, or continuation of the medullary substance of the brain and cerebellum. There are however, numerous facts which prove that its existence is independent of these parts. In speaking of the systems analogous to the spinal marrow in the lower classes of animals, they observe that, in the worms and caterpillars there are as many origins of nerves, or ganglions, as there are rings or segments; that these knots of cineritious matter are joined to each other by nervous branches; and form a nervous cord, studed at intervals, with small tumors of various forms and size but all divided into two parts, so that all is double. The number and size of the nervous threads proceeding from these swellings is always in a direct ratio to the size of the latter. In fishes, amphibia, and birds, the principle is the same: there is no essential difference; only the ganglions are in general closer to each other, forming with their uniting bands, a cord nearly equal in size, but swelling out in distinct knots wherever large nerves pass off; and if

closely examined we may perceive throughout alternate swellings and contractions. This is very visible in the fowl, each pair arising from cineritious substances, and communicating with those on each side by connecting branches. The same law obtains in mammalia and in man, although the facts are not so immediately evident. The alternate swellings corresponding to the origin of the nerves may be seen by removing the spinal marrow, detaching the arachnoid coat, and observing the profile when held against the light. The largest swellings are where the nerves of the extremities go off, but they are sufficiently visible throughout, the line being more or less undulating.

The interior structure of the spinal marrow, though often examined, does not appear to be well understood. There is evidently an anterior and a posterior longitudinal fissure. Our authors deny the existence of any lateral ones, in which they are certainly correct. The anterior fissure is more visible and larger, the posterior penetrates more deeply. The anterior fissure has also the peculiarity of being interrupted at the beginning of the medulla oblongata by the decussation of the pyramidal eminences. The two halves of the spinal marrow are united at the bottom of each fissure by a nervous layer, or commissure; the fibres in the anterior being transverse, and in the posterior directed lengthwise. They consider these nervous layers as the apparatus by means of which the two halves of this part of the nervous system communicate with and act on each other, in the same manner as takes place in the brain and cerebellum by their several commissures. This we think will be readily admitted.

It has been the common opinion that the spinal nerves are derived only in a secondary way from the spinal marrow, their original production being referred to the brain. They are here considered as originating in the cineritious substance of the spinal marrow. This substance is found in the central parts of each side of the cord, joining "two arched streaks, which pass from the middle layer towards the anterior and posterior surface, so that the two lines of each half form two arcs, the convexities of which terminate at the median layer." All the

nervous fibres follow the course of these striæ from within to the surface, throughout the whole length of the spinal marrow, so that we have on each side two rows of nerves, an anterior, and a posterior.

The nervous systems of the vertebral column serve to the brain as the instrument for voluntary motion, and the conductors for sensation. The same nerve is employed for each purpose, although by disease sometimes one power is lost and the other retained. The manner in which this is produced is not explained. The diseases of these systems are not well understood. The injury of one side of the spinal marrow is felt on the same side and does not necessarily extend its effects to the other; there is therefore no crossing of nerves. But there certainly is a communication between the origin of each pair; which fact may throw some light on the sympathies of corresponding parts on the two sides of the body.

The existence of a canal in the middle of the spinal marrow has been casually mentioned by several writers. Our authors give the following account of the appearance. They first observed it in examining a case of spina bifida attended with hydrocephalus:—

"In order to know positively if there existed any communication between the sac and the internal part of the spinal marrow, we divided the latter transversely in the neck: it exhibited its natural appearance. Blowing gently through a pipe on the cut surface, we perceived an opening as large as that of a common quill in each half of the spinal marrow, separated by the commissure. They were the openings of two canals, which, by dividing lower and lower down, were found to terminate about three inches above the end of the spinal marrow, having no communication whatever with the sac. Doubtful if this appearance might be the effect of disease, we examined this part in children and in adults. We found the same structure with this difference only, that in the adult and older children these canals did not open so freely as in the younger subjects. The structure is most easily shown in children just born. In other respects they are alike. If we continue to blow gently into these canals from below upwards; and if, after having opened by this operation the length of six or eight lines in the

spinal marrow, we only cut off four or five in order to maintain the opening, we may follow these canals through the medulla oblongata, the tuberculum annulare, the crura of the brain, and as far as what are called the optic thalami, in which they form a cavity as large as an almond, when blown up: they are closed at the anterior margin of the thalami, at the commencement of the corpora striata. They have no communication with the cavities of the brain."

Our authors give many sound reasons for considering these as natural canals, not formed by artificial expedients. Indeed there can be no reason to doubt a fact, which is made very apparent by gentle management. They are easily shown to within a short distance from the lower end of the spinal marrow.

At the end of this section several anatomical and physiological corollaries are introduced, containing the results of the inquiries into these parts of the nervous system. The principal points in this exposition of their opinion, and which they keep constantly in view throughout the whole work, are, that the cineritious, grey, or gelatinous substance every where produces the nervous fibres, and that it is essentially the same in the ganglions, the spinal cord, the cerebellum, and the brain, though somewhat modified according to the different character and functions of the nerves proceeding from it.

We are unwilling to venture a hasty opinion on these leading parts of the authors' doctrines, as they are deserving much sober investigation, and appear to have been formed with much caution, and after much laboured research on their part. The propriety of using the term origin has been often called in question, as conveying false notions with regard to the connexions between different parts of the animal system. The manner in which it is employed in the present instance has been much objected to, and the notion of the nervous fibres being produced from any part has been ridiculed as unphilosophical. Now it is not at all certain that every part is really formed at the same time, so that it would be incorrect to speak of one part arising from, being produced, or perfected by, another. It is evident that the nervous system is essentially composed of two substances: one of a grey colour, in general pulpy and gelatinous; the other white, firmer, and formed of parallel fibres. It

is also evident that we always find a collection of the first, more or less in quantity, at the beginnings of the nervous filaments. We do not say that one part is the cause of another, but we cannot see any absurdity in supposing the nervous fibres to be produced in and dependent on the cineritious substance. We know that, to follow the accepted language of physiology, we must say that every part is produced immediately from the blood. Perhaps however, this will be found to be yet too sweeping a conclusion. The identity, or rather similarity, in structure and functions between the cineritious matter of the ganglions and of the other parts of the nervous system, is the other point which our authors think they have demonstrated; but which appears to have met with as much opposition as their opinions on the production of the nervous fibres. We are aware of some objections: the strength of the argument however is certainly at present with our authors. We wish we may not chance to meet again with physiological doctrines less ably supported by facts and by reasoning than the present.

In the next section we have a dissertation on the differences between animal and organic life, "*la vie automatique*." They point out some errors in the beautiful theory of Bichat; some of his opinions not being borne out sufficiently by facts. For instance, the position—that the organs of animal life are double and precisely alike in size form and situation, is not rigorously true. It is not correct also, that the organs of animal life only arrive at perfection by frequent exercise; and that those of organic life are perfect at the first moment they are called into action. The truth is, that organs exercise their functions exactly in proportion to their development: where the organs are complete at the instant of birth, their functions also are perfect, under whatever class we may choose to arrange them. The error has arisen from taking too partial a view of the functions of the animal system. We are, perhaps, all too fond of generalizing from limited observations on the human structure, without looking abroad through the rest of the animal creation.

Section 4. The part which immediately succeeds the nervous systems of the spine increases considerably in size as soon as it becomes contained within the cranium. It is to this part that the authors trace the "first visible origin of the greater

number of the nerves of the senses, of the fifth, sixth, and seventh pairs, as well as of the brain and cerebellum. It has in common with the spinal marrow, the anterior and posterior fissures, the commissure or transverse layer of union, the internal layer of cineritious substance, and the canals made visible by blowing." It is usually called the medulla oblongata, and its limits have been variously fixed by different authors. The nerves proceeding from it are commonly termed nerves of the brain, as it appears, very incorrectly. The authors call them the nerves of the head. They consider them as nervous systems wanting in the inferior orders of animals, and designed for the functions of the senses. In the sympathetic and spinal systems, the nerves are speedily detached from the mass: here, the nervous bands remain attached to the mass for a considerable space. We must be careful therefore not to confound the point of origin with the point of departure from the mass. As long as the band continues united to the mass they think that it continually receives some augmentation from the cineritious matter contained in it. Frequently the cineritious substance is collected into a body, a ganglion, in order still further to reinforce the nervous fibres. The arrangement according to the point of departure from the mass is certainly therefore defective, as this is often distant from their real origin.

Our authors reckon eleven pair of the nerves, in accordance with the opinion of the best anatomists. The origins of most of these have been followed by them to the medulla oblongata. The accuracy of their conclusion is not we think to be doubted. Their discoveries are earnests of what may yet be done by a careful examination of parts, the structure of which is supposed to be fully known. They bear honourable testimony to the talents and labour of our authors, and should dispose us to a fair confidence in their assertions where from want of knowledge or address we may not be able at once to prove them true. We can readily suppose that there are some who will consider all their pains as idle and unnecessary, who cannot see what advantage is ever to accrue from such minute investigations. It is certainly but too true that in general our knowledge of real structure is very inaccurate, and is wholly insuffi-

cient to guide us without error in examining the healthy or disordered functions of parts. But who is to say that we can never arrive nearer perfection, or that an intimate knowledge of organization is not the first step towards reaching it. The want of precise knowledge has ever been the greatest obstacle to improvement. For ourselves, and in respect to the present subject, we must say, that whoever has witnessed the careless routine in which the nervous system is shown in our anatomical schools, the exhibitions of melting brains and shrivelled preparations, could not be surprised if we were to remain for ever in the dark as to its structure and functions.

There can be no dispute about the origins of the accessory nerve, the hypoglossal nerve, the vocal nerve or nervus vagus, and the glosso-pharyngeal nerve. They bear a close resemblance in this respect to the nerves of the spine. "The following have this peculiarity, that the threads which are successively produced in the cineritious substance unite into one band before they quit the mass; which band is distinctly directed upwards, sometimes more, sometimes less visible, according as it is more or less covered by the neighbouring bands." It will not be within our limits to follow the authors through this part of their labours. The reader will find their opinions, expressed with sufficient correctness, in the report delivered to the Institute referred to in our last number. There is some slight difference, but not enough to affect the general result. "It is certain that all these nervous systems originate in cineritious substance, and by no means in what is called the medullary substance of the brain. It is also demonstrated that the different nervous systems are not perfect in the first instance, but are strengthened and perfected during their course. For this purpose, they are in some instances accompanied in their passage by the cineritious substance, increasing in size the further they are from their origin. In some cases the nerves in their passage cross a collection of cineritious substance, the latter producing additional nervous threads which unite to those already formed, and so on until the system is completed."

Section 5. On the difference of nerves. They consider the nerves as different throughout. There is a primitive difference in their internal structure, and an essential necessity therefore

for a difference of functions. They all originate in different masses of cineritious substance, and have often an apparent difference in structure. Our authors occupy themselves, we think rather idly, towards the end of the section, in refuting at length some marvellous opinions on the subject of animal magnetism, which it should seem are still in fashion in some parts of the continent.

In the next section they enter at great length on the functions of the external senses. The subject is handled with great ingenuity and exhibits many proofs of originality of thought and of accurate reasoning. One of their objects is to prove the perfection of each organ of sense individually considered, and the futility of the speculations of many eminent philosophers who argue for the necessity of an exertion of the judgment, and a combined action of different organs to make the sensation perfect. We look forward to no little instruction in seeing the same extensive spirit of research at work on the functions of the brain. Their remarks on the senses of sight, hearing, and touch are particulary interesting, and will correct many false notions which have crept into physiology from the philosophical writings of our most distinguished metaphysicians. This class of authors have certainly broached many incorrect, and some absurd opinions from attending too much to the sensations in man, and from an ignorance of many points in the comparative structure and functions of man and other living creatures. If all the physiological lectures of our authors were like these, we can readily account for their popularity.

They next proceed to the examination of the cerebellum and the brain. To avoid any confusion they consider the systems of organic life, those of voluntary motion, and of the senses, as entirely distinct; and only comprise under the term brain the nervous systems, which they suppose to be the special organs of the moral and intellectual qualities. It is placed in communication, and in reciprocal action and reaction with the other systems of the nerves. It no more arises from the nervous mass of the spine, than it gives origin to it. Like all the rest of the nervous systems it is composed of two substances, the grey and the white, or of cineritious matter and nervous fibres. The fibrous nature of the last

in the brain they consider as a point in structure abundantly demonstrated. The word medullary therefore, they wholly lay aside. In the common mode of exhibiting the organization of the brain by thin slices this appearance is scarcely visible, but by gentle scraping in the course of the fibres it is at all times *apparent*. Where from their superficial position the fibres are exposed to view without any preparation this structure is equally apparent. The cineritious matter is variously disposed, but its nature and offices are every where the same.

Most of the nerves of the senses have their first visible origin in the cineritious substance of the medulla oblongata, "*le grand renflement;*" it is to this part also that our authors trace the first visible origin of the cerebellum. These origins form on the outer side of the medulla a large band, the *corpus retiforme* or *processus cerebelli ad medullam*, which enlarges as it mounts, the fibres passing into the interior of the cerebellum. A short way after their entrance they meet with a mass of cineritious substance, the *corpus ciliare*, with which they become so intimately united as to render it impossible to follow their direction. This cineritious matter is the ganglion of the cerebellum and gives origin to many new fibres. The bands which pass out ramify into the various subdivisions of the cerebellum. One of the principal bands passes towards the median line, and contributes with the corresponding one of the opposite side to form the middle part of the cerebellum. This is a fundamental part found in all animals, whatever be the disposition of the brain in other respects. The lateral parts are superadded in different animals in various proportions. All these nervous fibres are covered at their peripheric extremities by cineritious substance. To exhibit these appearances nothing more is necessary than to make a vertical section in the direction of the primitive band so as to divide the ganglion into two equal parts.

So far they describe the nervous fibres diverging, and occupying a larger circumference. There is also another order of fibres which arise from the cineritious matter of the surface, and pass in the intervals of the fibres first described, which they cross, always converging towards the external and anterior margin, where they form a broad and thick fibrous

layer. The posterior and middle filaments pass transversely through the longitudinal bands of the medulla oblongata, which go to diverge in the hemispheres of the brain. The anterior filaments lie in front of these bands, in the form of a broad fibrous layer in man. They all unite in the middle line with their fellows of the opposite side, thereby joining the two hemispheres of the cerebellum, after the same manner as the hemispheres of the brain are united by the corpus callosum, &c. This part which now goes under the name of the pons varolii, or tuberculum annulare, is really then the great commissure of the cerebellum; and is always in a direct ratio to the size of the hemispheres, as the primitive bands are to that of the ganglion. The mammalia having smaller cerebella have also this commissure smaller. In man, where the cerebellum is so large and so fully developed, the commissure is particularly broad, covering the origins of many of the nerves which are exposed in other animals. This commissure is not found, or there is not, to use the common language, any pons in fishes, reptiles, and birds: for these animals have no lateral parts to their cerebellum, and consequently no converging fibres to form it. But all animals have a commissure of what our authors call the fundamental part, independent of the commissure of the lateral parts, or pons. This commissure is formed by the soft and fine fibrous layers from the superior and inferior parts of the middle or fundamental portion, commonly called the superior and inferior valves.

The cerebellum does not contain more cineritious substance in proportion to its size than the brain. The mistake arises from our generally making horizontal sections of the last, and vertical ones of the cerebellum; reverse them and the brain will appear to contain the most cineritious matter. The cerebellum is not always in proportion to the brain, as to size or development: it is not so large proportionally to the brain, in children as in adults.

In describing the brain, our authors confine themselves principally to the human subject, as the one in which the organization of the brain is the most complete, and of which the different parts can be represented in the most sensible manner:

"The brain consisting of many divisions, the functions of

which are totally different, there are found many primitive bands which by their development contribute to form it. All these bands are composed gradually by fibres produced in the cineritious substance of the medulla oblongata (*du grande renflement.*) They are to be considered as the first visible rudiments, or commencement of the brain, having communication and reciprocal actions with the surrounding systems. Among these primitive bands we place the anterior and posterior pyramidal eminences, the bands which proceed from the corpora olivaria, the longitudinal nervous bands which contribute to form the fourth ventricle, and some others which are concealed in the body of the medulla oblongata."

With but one exception the bands pass into the hemispheres, keeping the same side in which they originate. With regard to the anterior pyramidal eminences the case is reversed. Here the fibres which arise on the right side unite in the first instance into a few cords which pass obliquely over to the left. In the same manner they pass from left to right, each cord passing over one and under another of the opposite side forming a sort of interweaving like basket-work. This decussation occupies a space of four or five lines at the lower extremity of the medulla oblongata. The bands afterwards pass up on the anterior surface of the medulla, gradually acquiring the pyramidal form. This appearance is very easily demonstrated, and is totally distinct from the transverse layer or commissure, visible both above and below this part in the anterior fissure of the medulla. The authors hint that as the fibres of the anterior pyramids are the only ones which decussate, so it is only an injury of that part of the brain which is a continuation or development of these bodies, which affects in its consequences the opposite side of the body. Injuries of the cerebellum, of the posterior lobe, and of part of the middle convolutions of the brain, would affect the same side, these parts being continuations of fibres which do not cross. This is an interesting point and deserving of further research.

The pyramids as soon as they enter the tuberculum annulare divide into many bands, which lie in a considerable mass of cineritious substance: from this many new fibres arise and join them. It is regarded as a true ganglion. The bands are

so much enlarged when they pass out above as to form nearly two thirds of the crura cerebri, "*les grands fasceaux fibreux.*" The transverse nervous bands seen in the tuberculum annulare, are, as before mentioned, the commissures of the cerebellum. This structure may be seen by making a vertical incision curved in the direction from the pyramids to the crura cerebri. The great fibrous bands contain in their interior a considerable quantity of cineritious matter, from which they are continually reinforced by new fibres. The nervous fibres are continued upwards, and gradually terminate in forming a nervous layer, covered by cineritious matter externally. "Thus the pyramidal bands may be traced from their point of origin until they are perfected and terminate in the inferior, anterior, and external convolutions of the front and middle lobes. These parts of the brain are always in proportion to the pyramids, the cineritious matter of the tuberculum annulare, and the crura cerebri in other animals, as well as in man." The same principle of structure is followed in the formation of the posterior lobe, and of the convolutions situated at the upper and internal edge of each hemisphere. The corpora olivaria they consider to be ganglions. From each there ascends a large band, joined with the posterior bands from the medulla oblongata. They cross the tuberculum annulare and form the posterior and inner third of each crus. They receive but little addition until they enter a thick mass of cineritious matter, known generally by the name of thalamus opticus. In this ganglion is to be seen a large quantity of nervous filaments, which ascend, and at their exit upwards unite into bands, which immediately diverge in a radiating form and traverse another large mass of cineritious substance, the corpora striata. In this ganglion they are again reinforced and eventually form the posterior convolutions and those placed internally at the upper part of each hemisphere.

The canals mentioned as existing in the nervous mass of the spine are prolonged between the two ascending divisions of nervous fibres just described, following the course of each crus. At the part where the fibres pass out from the thalami there is so firm a union between these and the transverse con-

verging fibres which go to form the commissures, as to render all further separation impossible without laceration.

The thalami optici and corpora striata are in this view considered as true ganglions, in which the nervous fibres are increased in number and size, and are prepared by degrees to form their respective convolutions. The latter are regarded as the perfect organs, and the preceding as the preparatory apparatus. "In examining under a general point of view the manner in which nature proceeds to the primary formation of the two hemispheres of the brain, we see that the original bands are produced in the cineritious substance of the medulla oblongata, and reinforced in different places by particular masses of cineritious substance; that consequently all follow the same laws as are observed in the nervous systems of the abdomen and the thorax, of the vertebral column, of the nerves of the head, and of the cerebellum."

All the parts of one side of the brain communicate with corresponding ones of the opposite side by transverse, or as they generally call them, by converging nervous fibres which unite at the median line. The ascending or diverging fibres have been followed into the cortical or cineritious substance, which they penetrate to some depth, making the internal part of a lighter grey than the external. How they finally terminate is not positively stated. It is thought probable that new nervous fibres are produced in the cineritious matter intimately connected with those whose course we have been following. The existence of the converging fibres may be more readily demonstrated than even that of the diverging. They may be seen at the bottom of every convolution, decussating the latter, with which they are intermingled. As they advance inwards they form nervous bands and layers which go out of one hemisphere to meet corresponding ones from the other, forming the different junctions or commissures. The principal converging bands are the corpus callosum, the fornix and lyra, and the anterior and posterior commissures. The size of the different bands of reunion is always in proportion to that of the parts they unite in the different classes of animals.

"The brain is then formed, and perfected in the same

manner as the other nervous systems. The analogous parts of the two hemispheres have, as the analogous nervous systems of each side of the body, their reunions in the median line. Who in the conformity of these laws can mistake the character of truth? Can any one still prefer the arbitrary, confused, and incoherent method of studying the brain to our method, so simple, sure, and so well followed up?"

The authors proceed to describe the different cavities of the brain, and the structure of the convolutions. The diverging fibres, after decussating the converging fibres at the outer side of the great ventricles, which they thus effectually close, divide from each other more and more, and terminate by forming a fibrous expansion. They are unequal in length; some ending immediately beyond the outer sides of the ventricles, others continuing further; the interior having the longest course. The nervous fibres are thus lost successively in the cineritious substance, the middle ones only being continued to the extremity, and forming two layers touching each other in the middle of each convolution, slightly agglutinated by a mucous or fine cellular tissue. They are covered throughout by a layer of cineritious substance of nearly uniform thickness. The same takes place in the cerebellum, but the divisions are more minute. Each convolution then is composed of converging and diverging fibres, and of a cineritious envelop. From the structure just described arises the possibility of separating the two layers without injuring either. The division in each convolution is easily demonstrated by blowing gently, or directing a small stream of water on the middle line. The fact is rendered still more evident if the part be hardened in nitrous or muriatic acid diluted with alcohol, or if it be boiled in oil. If the part at which the different fibres decussate at the outer side of the ventricles is ruptured, the whole of each hemisphere may be unfolded as it were, and made to appear as a soft bag, white and fibrous on the internal, and grey on the exterior surface. The pineal and pituitary glands are considered to be ganglions, as are also the corpora albicantia. Our authors treat also of many minor points of structure in different parts of the brain which we must wholly omit notic-

ing, hoping that enough has been given to convey to the reader some notion of their principal opinions.

In this imperfect sketch we have passed over the reasons given by Gall and Spurzheim in support of their physiological opinions, many of which are very forcible: we have also omitted the answers given to different objections made to several parts of their doctrine. If we have not expressed any opinion as to the merits of the physiological doctrines introduced throughout this work, it is because we do not think ourselves qualified at once to decide on them, and because they rest on facts the existence of which may be ascertained by any of our readers. The anatomical details will be found strictly correct. From a want of rightly comprehending the authors' meaning, or of sufficient address, it may happen that a superficial examiner may be disappointed in his search. The fault however will be with him. There is never any ambiguity of expression, or any confusion in the descriptions; they are all lucid, and rather diffuse. The latter circumstance we think an advantage in describing structure which is delicate, and on which there has been so much misunderstanding. Conciseness is often overrated, and more than counterbalanced by the want of perspicuity which frequently attends it.

Whatever be the ultimate fate of their physiological opinions, anatomy must always gratefully acknowledge the labours of our authors. The great mass of anatomical information collected in this volume makes it highly valuable. They have we think taken the true route, and one unknown to, or imperfectly seen by any of their predecessors. They have made considerable advances; but the termination is yet far in the distance, and it will require long continued and extensive research before we can hope to reach it. Whilst anatomy possesses such intelligent and indefatigable labourers as Gall and Spurzheim, and men endowed with such ample talents as Sœmerring and Cuvier, we may yet expect great things.

The subject is one of the highest interest; and the prosecution of inquiries into the organization, functions, and diseases of the brain and nervous system, is the more to be encouraged as it is notorious that the sum of our present knowledge is

trifling indeed in amount, when compared with what is yet wanting. We cannot help lamenting the apathy of many who neglect, and the absurdity of others who ridicule such labours as the present. Few and of little import are the additions made of late years to physiological knowledge by the anatomists of this country. The appearance will be still more meagre if contrasted with the rich discoveries made by some of the ardent anatomists of the continent. It would be out of place to enter here on the causes of this deficiency on our part: the two principal ones are the total neglect of the higher parts of physiology in our anatomical schools, and the almost exclusive encouragement given to surgical anatomy. We may well prize our chemical, but we have no cause to boast at present of our anatomical philosophers.

The plates are without exception the most beautiful and the truest anatomical pictures of the brain which we have ever seen. The magnificent plates of Vicq d'Azyr appear hard in the comparison, and they are in many minute points confessedly inaccurate; and from their being composed from views chiefly of horizontal sections manifestly insufficient. The beautiful plate of the basis of the brain by Sœmerring, the best we possessed and by the most perfect anatomist of the present day, is not we think equal to a corresponding plate in the present work. They have another great merit also besides that of being well executed,—that of being well contrived to exhibit the structure of the parts they represent.

ORIGINAL PAPERS.

On the separation of the colouring matter of the Coccus Cacti *or Cochineal of a* Lilac *colour, by the decomposition of acetite of lead and sulphate of zinc. By* JAMES CUTBUSH, *Professor of Chemistry, Mineralogy, and Natural Philosophy in St. John's College.*

THE four following experiments and observations were drawn from a phenomenon, which accidently occurred on mixing acetite of lead and sulphate of zinc with cochineal. The peculiar colour which was produced struck my attention the more forcibly, as it is well known that several of the metallic salts have the property of separating the colouring matter of a red, and more frequently of a crimson colour. I found, that when acetite of lead and cochineal were mixed, no phenomenon ensued; but that when sulphate of zinc was added, an immediate decomposition was the consequence; and the colouring matter of the cochineal was precipitated, not of a red, but of a lilac colour. I was anxious, therefore, to determine whether the effect ensued by the agency of the acetite of zinc, or by that of the sulphate of lead, which were formed in the process. To ascertain this circumstance, the following experiments were instituted.

Experiment 1. Acetite of lead triturated with cochineal and water, afforded a purple colour.

Exp. 2. Sulphate of zinc mixed in a similar manner, gave a dull red colour.

Exp. 3. Acetite of zinc produced no material alteration. The effect however, was more like that of the solution of tin.

Exp. 4. Cochineal triturated with sulphate of zinc and acetite of lead, produced a lilac coloured precipitate with water.

These few experiments at once determine, that neither the acetite of lead, the sulphate nor the acetite of zinc had any agency in changing the colour of the *colouring matter;* but, that the sulphate of lead, at the moment of its formation, by the double decomposition which ensued, had the effect of

altering it so as to destroy its original colour, and of precipitating it of a lilac hue. On drying the precipitate, the colour appeared uniform; and so far as I may judge, seems to be a chemical compound of the sulphate and colouring matter. From the presence of the sulphate, as a body or base, and from the uniformity of the colour, I think it probable that it might be employed advantageously as a pigment for miniature painting; but, with respect to its durability, I am not yet determined.

As to the property which certain earthy and metallic salts possess, of precipitating the colouring matter of cochineal, for the operations of the dyer, it is well known that the chemical experiments which have been instituted on the subject, have led to better processes, and more correct and more desirable results. But, as regards the changes which take place in certain processes, not only in the substances themselves, but in the rays of light, in order to produce colour it is necessary to obtain a knowledge of all the circumstances concerned, to take into consideration the affinities or relations which such substances, or compounds so formed, have to their constituent parts, as well as to the rays of light. Bancroft, Berthollet, and more lately, Dr. Conover (in the last volume of the American Philosophical Transactions,) have considered this subject with becoming attention, in which they have paid every respect to the doctrine and opinions of sir Isaac Newton.

It is not our intention, however, to enlarge on this subject, nor to notice the theories and opinions which have been formed; but only to add, that an acquaintance with such branches of optics as relate to the laws and properties of light, with its connexion to chemical subjects, will lead to more correct deductions on the subject of colours. Dr. Conover, in a very able and ingenious essay already quoted, has shown the importance of such knowledge as applied to the arts; and has treated the subject, with its connexion to the arts in so clear a light, that but little remains to put his principles into practice. I allude to the formation and fixing of certain colours on porcelain, &c. by the agency of certain *determinate* temperatures. In the phenomenon before noticed, the production of the lilac colour, and indeed the violet and purple colours, will be found

to be compounds of red and blue in different proportions, so that a knowledge of the laws and properties of light must eventually lead to correct deductions, and show how far certain rays are concerned. These opinions have been ably supported by Mr. Dalton, in an essay on the prismatic colours. Considering the lilac a compound colour, might we not obtain a *blue* pigment by using either less of the cochineal, or a larger quantity of the sulphate of zinc and the acetite of lead, by which the red colour might be altogether discharged? This experiment I intend to perform at a subsequent period.

The further we carry our investigations, the more we perceive it necessary, in order to come at the truth, to consider the primary causes which operate in effecting the different changes; and, of course, the union of some, and the extrication of other principles, or constituent parts, to the accomplishment of different processes. These facts are the more interesting where a number of substances are concerned; where complicated affinities take place; and where the results are governed, for the greater part, by the interposing medium of light and caloric. If the production of colour be mechanical, which is contrary to fact, such changes in colour must have no relation with chemical changes; but, as chemical changes, in a variety of cases, are followed by a corresponding change in the rays of light (more especially in dying) we may establish it as a *datum*, that chemical results are affected by *all* the circumstances which can in any manner influence them. Colours it is true are formed in the solar light; the various tinges of colour resulting from the absorption of some of the rays of light, and the reflection of others. By the art of dying, therefore, a substance possessing the property of reflecting particular coloured rays, according to Bancroft, is transferred to the surface of another body. The preparation of the stuff, previously to receiving the colour, the menstruum or vehicle through which the colouring matter is transferred, and the different affinities which take place in these processes, show at once the truth of what we have advanced. But as this is irrelevant to our present subject, we shall not enlarge upon it.

INTELLIGENCE,

Medical and Philosophical.

On the Structure of the Eyes both of Man and Birds.

A PAPER by Mr. Champion was read before the Royal Society, on the structure of the eyes both of man and birds, particularly in relation to that faculty of the eye which enables it to adjust its focus to the distance of the object. The author examined the different conjectures and theories which have been proposed to account for the circumstance, and explain how the eye can have perfect images of objects at very different distances. It has been generally agreed, that the eye must have some contractile power; but the existence of any organ capable of such a function has never been ascertained. Mr. C. in examining the eyes of an eagle, was led to discover the existence of a small muscle attached to the sclerotica and capable of contracting the eye, in a manner equal to effect the necessary change in the focal distance. The same organ he discovered in some other birds, and hence he inferred that something analogous exists in the human eye. He observed, that images pass before the eyes of maniacs as vividly and distinctly without any sensible objects, as they do over those of some persons from objects within the focal distance of their eyes.

Experiments on Light.

A paper by Count Rumford was also read, on the nature of light. The count, firmly persuaded, that it is of great commercial importance to increase the quantity of light for men's use, and that this luminous *nothing* has no more distinct existence than sound, proceeded to make a number of little experiments on wax tapers and a photometer. He began by weighing the combustible matter consumed or transformed during a given time, and compared the quantity of light emitted in proportion to the wax burnt. In nine such experiments he satisfied himself that the light emitted bore no invariable pro-

portion to the quantity of combustible matter consumed, but that, with small tapers, which yielded very little light, there was a very considerable increase of heat. Here he was induced to make some observations on the nature of heat, or heated bodies, so far as their luminous qualities were concerned; all of which, he thought, tended to confirm his opinion, that there is no such matter in existence as light, and that philosophers may for ever torture their imaginations about its nature and qualities, without ever being a tittle the wiser. He observed, that no person ever looked for the nature and properties of sound in fulminating powder, and it is equally idle to look for those of light in combustible bodies. He has however a very philanthropic and important discovery to make, namely, a new invention of his *own*, a polyflame lamp, consisting of a number of burners, with wicks flat like a ribbon, and so placed one at the side of another that the air can pass between them, at the same time that they are duly supplied with oil. The flat wicks arranged in this manner, side by side, supplied with oil, and covered with a large glass which rose several inches above the flame, yielded as much light as forty candles! The count concludes with expressing his own liberality in thus publishing his discovery, and declares himself ready to give every possible information in his power to any person who may wish to construct such lamps; but modestly adds, that his own is not made quite so well as he could wish, and that his apparatus is still capable of further improvement.

January 23, 1812.

THE Jacksonian Prize of the Royal College of Surgeons in London, for the year 1811, has been adjudged to Mr. Joseph Hodgson, of Bucklesbury, London, for his dissertation on the diseases of Arteries and Veins; comprising the pathology and treatment of Aneurism and wounded Arteries.

Professor Leslie's Discovery.

THE experiments of professor Leslie, to produce ice by evaporation in the air pump, have been varied and extended in France by Messrs. Clement and Desormes: they have pro-

posed to apply the evaporation, in vacuo, on a large scale, to the drying of gunpowder; which, being done without fire, will be attended with no danger.

The French chemists are engaged in endeavouring to apply the evaporation in vacuo (before stated) to the drying and preserving fruit and vegetables. It may be easily conceived of what advantage this process may be, particularly in the army and navy, by preserving, unchanged, alimentary substances, and also by diminishing their weight and bulk, when they are to be sent to distant parts of the world.

In consequence of this paragraph, we observe that Mr. Leslie has addressed a letter to the editor of the Caledonian Mercury, and we can bear testimony to the truth of the professor's claims to the discovery; for we personally witnessed the demonstration of his theory while in London last year, when he freely explained the various important uses to which the principle might be applied, particularly by the introduction of the apparatus into hospitals in sultry climates, and for its general utility in domestic purposes. We understand that he has, since his return to Edinburgh, improved his coolers, so as to produce ice in large quantities, by powers much inferior to those he first used.

PENNSYLVANIA HOSPITAL.

At a meeting of the Contributors to the Pennsylvania Hospital, held on the 4th of May, 1812, the following Contributors were elected officers of that institution, for the ensuing year.

MANAGERS.

Samuel Coates,	Edward Pennington,
Pattison Hartshorne,	Samuel W. Fisher,
Joseph Lownes,	Thomas P. Cope,
Joseph S. Morris,	Zaccheus Collins,
Thomas Stewardson,	Reeve Lewis,
Lawrence Seckel,	Joseph Watson.

TREASURER.

Joseph S. Lewis.

At a meeting of the Managers, held on the 11th of May, 1812, the following Contributors were elected Physicians of the House, viz.

Thomas Parke,
Benjamin Rush,
Philip Syng Physick,
Benjamin Smith Barton,
John Syng Dorsey,
Joseph Hartshorne,
Thomas C. James,
for the Lying-in Department.
Samuel Calhoun, for the out Patients.

At a meeting of the President and Directors of the Pennsylvania Academy of the Fine Arts, May 11th, 1812,

Nathaniel Chapman, M. D. was elected Professor of Anatomy in the said Institution.

WM. SMITH, Sec'ry of the Academy.

At a meeting of the Philadelphia Medical Society, the following Members were elected Officers for the ensuing year.

PRESIDENT.

Benjamin S. Barton, M. D.

VICE-PRESIDENTS.

Philip S. Physick, M. D.
N. Chapman, M. D.

SECRETARIES.

Joseph Hartshorne, M. D.
A. Benezet, M. D.

ORATOR.

Charles Caldwell, M. D.

METEOROLOGICAL OBSERVATIONS.

State of the weather, &c. at Philadelphia during the first six months of 1812.

JANUARY.

Thermometer—Lowest, at 8 A. M. 6. 22d day of the month.
Highest, 3 P. M. 41. 31st.
Mean . . . 28.

Winds—Chiefly west and northwest.

January has been a real winter month throughout—the 22d day of the month was colder than it has been since December 23d, 1796—but one snow of any consequence on the 18th, when the river was frozen—not much rain this month.

FEBRUARY.

Thermometer—Lowest, at 8 A. M. 14. 12th day of the month.
Highest, 3 P. M. 45. 8th.
Mean . . . 30.

Winds—Very changeable—mostly northerly.

February has appeared in its usual form, changeable and unpleasant—three or four snows one or two inches deep—not much rain. The ice in the river broke up on the third. The winter continued severe to the end of the month. Frequent earthquakes to the southward and westward.

The measles which were observed last autumn, now more prevalent—the first case recorded on the books of the Philadelphia Dispensary, October 19, 1811, in the northwest part of the city—the disease is mild with the usual symptoms. Whooping cough also prevalent.

MARCH.

Thermometer—Lowest, at 8 A. M. 22. 2d day of the month.
Highest, 3 P. M. 60. 27th.
Mean . . . 38.

Winds—Very variable from all points of the compass.

March has been a cold and very unpleasant month, promising a backward spring—little rain.

On the 26th a dreadful earthquake felt on the Spanish Main, by which the cities of Caraccas, Laguira and several others were destroyed.

APRIL.

Thermometer—Lowest, at 8 A. M. 32. 12th day of the month.
Highest, 3 P. M. 75. 19th.
Mean . . . 45.

Winds—Much easterly.

April, on the whole, has been a cold and dry month, unfavourable to vegetation. The 19th was warmer than any day in April that is remembered, although the season was in general cold.

MAY.

Thermometer—Lowest, at 8 A. M. 42. 5th day of the month.
Highest, 3 P. M. 75. 31st.
Mean . . . 50.

Winds—Chiefly eastwardly.

Snow on the 4th, and ice on the 5th. More frequent rains—cold season and backward vegetation.

The newspapers give an account of a considerable volcanic eruption of Mount Souffriere in the island of St. Vincents on the first day of this month.

JUNE.

Thermometer—Lowest, at 8 A. M. 61. 5th day of the month.
Highest, 3 P. M. 82. 8th.
Mean . . . 70.

Winds—Northerly and westerly.

Heavy rains and thunder on the 8th. Heavy rains on the 16th and 24th, also on the 27th with very high wind and loud thunder—good prospects of harvest—the weather unusually cool.

The measles continued to the end of this month—little or no small-pox in the city for the last six months—vaccination generally practised.

RECENT FOREIGN PUBLICATIONS.

Dissertation on the Bite of a Rabid Animal. By J. Gilman.

Practical Observations on Debilities. By J. Caton.

A View of the Nervous Temperament, being a practical treatise on nervous, bilious, stomach, and liver complaints, &c. By T. Trotter, M. D. Third edition, with large additions.

Observations on the Diseases of the Liver. By T. Mills, M. D.

A Treatise on the Management of Infants, &c. By J. Syer, Esq. Surgeon.

Essay on Scrofula. By J. Armstrong.

The Surgical Anatomy of the Head and Neck. By A. Burns.

On the Operation of largely puncturing the Capsule of the Chrystaline Humor, in order to promote the Absorption of the Cataract. By J. Ware, F. R. S.

A Treatise on some practical points relating to the Diseases of the Eye. By the late John C. Saunders.

The Principles of Physiological and Physical Science, &c. By Richard Saumarez, Esq. Surgeon.

Observations on the Contracted Intestinum Rectum and Mode of Treatment. By W. White.

Review of Mr. E. Home's Practical Observations on the Diseases of the Prostate Gland. By Jesse Foot.

An Examination of the Mineralized Remains of Vegetables and Animals of the Antediluvian World, &c. By J. Parkinson. Vol. 3.

General Zoology, &c. By George Shaw, M. D. F. R. S. Vol. 8. being the 2d vol. of Birds.

Essays on Natural History and Rural Economy. By T. Walker.

PROPOSED FOREIGN PUBLICATIONS.

Dr. Cheyne of Dublin, has in the press, a curious and extensive work on Apoplexy, with plates illustrative of the disease.

Sir Humphrey Davy has in the press, a volume of the Elements of Chemistry.

Dr. Stokes of Chesterfield, has just finished, in 4 vols. his Botanica Medica.

Mr. Adams has in the press, a Treatise on the Morbid Affections of the Eye and its Appendages.

Dr. Sutton is preparing for publication, Observations on the injurious Effects of Mercury in various diseases.

Dr. Lipscombe has nearly ready for the press, a Grammar of the Elements of Medicine.

Dr. Delys of Birmingham, has in the press, a translation of Richerand's Elements of Physiology, from the 5th and last edition illustrated by notes, and accompanied with a comparative view of the state of physiology in England and on the continent.

Professor John Leslie of Edinburgh, has in the press, a View of the Facts ascertained concerning Heat, and its relations with Air and Moisture.

RECENT AMERICAN PUBLICATIONS.

An Account of the New York Hospital. New York, printed by Collins & Co. 1811. 8vo. To which is appended, a Catalogue of Books belonging to the New York Hospital Library.

An Essay on the Organic Diseases and Lesions of the Heart and Great Vessels. From the clinical lectures of J. N. Corvisart, first physician of their imperial and royal majesties; officer of the school of medicine of Paris, and of the imperial college of France; physician in chief of the hospital of La Charité, &c. &c. published, under his inspection, by C. E. Horeau, doctor in medicine, surgeon of the infirmary and house of the emperor and king. Translated from the French, with notes, by Jacob Gates, M. M. S. S. Philadelphia, Anthony Finley.

A Dissertation on the Use and Abuse of Tobacco, wherein the advantages and disadvantages attending the consumption of that entertaining weed, are particularly considered. Humbly addressed to all Tobacco Consumers, but especially those among Religious People. Second American edition. By Adam Clarke, L. L. D. Newburyport, Thomas & Whipple.

Original Communications for this work will be thankfully received by Thomas Dobson, No. 41, south Second-street.

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